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ARNOLD C. KLEBS NUMBER

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Fig. 1.

Edwin Klebs and Rose (Grossenbacher) Klebs,
the parents of Arnold C. Klebs.

Courtesy of Dr. Leona Baumgartner.

CONGRATULATORY EPISTLE TO ARNOLD C. KLEBS

Dear Arnold:

And so it has become true. You are actually seventy years old today. You have had your three score years and ten and as you wrote the other day quoting Harvey Cushing, "We have had a long and genial run for our money, so there is nothing to regret when we go." You have not been cheated. The average life expectancy in the United States and in Switzerland is around sixty years. You have had them and ten in addition and we hope that you shall have many more. Yes, I know what you are going to say, that you do not want an "opium cum digitali," an old age in decrepitude and gagaism. But you are subject neither to the one nor to the other and you have not finished your work. Besides, in these days when so many people are dying without knowing what for, it is good to be alive:

> Μὴ δή μοι θάνατόν γε παραύδα, φαίδιμ' Ὀδυσσεῦ.
> βουλοίμην κ' ἐπάρουρος ἐὼν θητευέμεν ἄλλῳ,
> ἀνδρὶ παρ' ἀκλήρῳ, ᾧ μὴ βίοτος πολὺς εἴη,
> ἢ πᾶσιν νεκύεσσι καταφθιμένοισιν ἀνάσσειν.

It must be lovely at Les Terrasses today. The winter was unusually cold but now spring is in the air. The garden is full of crocus, snow-drops and "Märzeglöckli." The forsythias are in bloom and the lake is losing its gray color and is becoming green and blue. Boats are being repainted and soon the lake will be dotted with the white triangles of the Voile Latine. You know, it really is good to be alive, if for no other reason than to experience every year this miracle of spring, the rebirth of nature, l'irrésistible poussée de la sève, Adonis returning from the Underworld, back to the light, back to Aphrodite—the great continuity of life.

It makes little difference to the world whether we have been in it or not, but it is a great thing for us to have been a link in the chain, to have participated in the continuity of life, procreating children, physical and spiritual children, contributing our share, infinitesimal as it is, to progress. Now the word is out. Do not argue, I know

Fig. 2.

American Pilgrimage to Louis' Tomb, Mont-Parnasse, October 1905

Left to right: Klebs, Kayserling, Beyer, Whitman, McCarthy, Norton, Osler, Pearson, Magnin, Jacobs, Knopf, Evans, Lowman, Brennan, Pottenger.

your objections, I have made them myself. But I believe in progress, in the deep-rooted aspiration of man to develop a better world with more justice, tolerance and kindness. It is hard to keep one's faith today in a world in which the most savage instincts are loose. But civilization is in its infancy still. What are a few thousand years of history compared to the half million years when man lived without history like a beast in the forest. And who knows? You were born a few months before the Franco-Prussian War broke out and again the world is aflame. It was perhaps our lot to live through the birth-pains of a new world.

We, your friends, wish we could spend the day with you. We would walk through the garden to the library. Over the door are Leonardo's words " Se tu sarai solo tu sarai tutto tuo." You would not be " tutto tuo " but " tutto nostro " on such a day. We would sit in the study and would indulge in that finest of arts, the art of conversation. You would shoot your paradoxes at us, would tell us that bacteriology is a superstition, vaccination a fake, and that the best than can happen to a patient is never to see a doctor. We would shoot back and we would discuss everything under the sun from Greek mythology to incunabula and the philosophy of sulphanilamide, and would have a hell of a good time. Then we would enter our names into your guest book, the panelled wall of your study, and would leave refreshed in high spirits feeling that A.C.K. is sometimes a little bit difficult, sometimes a little bit crazy, but a grand guy to whom we shall always be deeply attached.

Like everybody you were born with certain handicaps. One was that you were the son of a famous father. I remember that once in Zurich, years ago, I introduced you to a colleague as " Dr. Klebs, the son of Edwin Klebs," whereupon you remarked that it was sad to have reached the age of fifty and still be introduced as the son of one's father. Your father was a Prussian, your mother Bernese, " es Emmetaler meitschi." Both came from sturdy races, where people have hard skulls. You got one in the cradle. Hard skulls are liable to make bumps and to become bumped. As a student you fought duels, and later—well, I was not the only one with whom you had a quarrel. But this was long ago. Natures soften with time and after all—without sparks there would be no fire.

I think that all in all life has been very good to you. You were

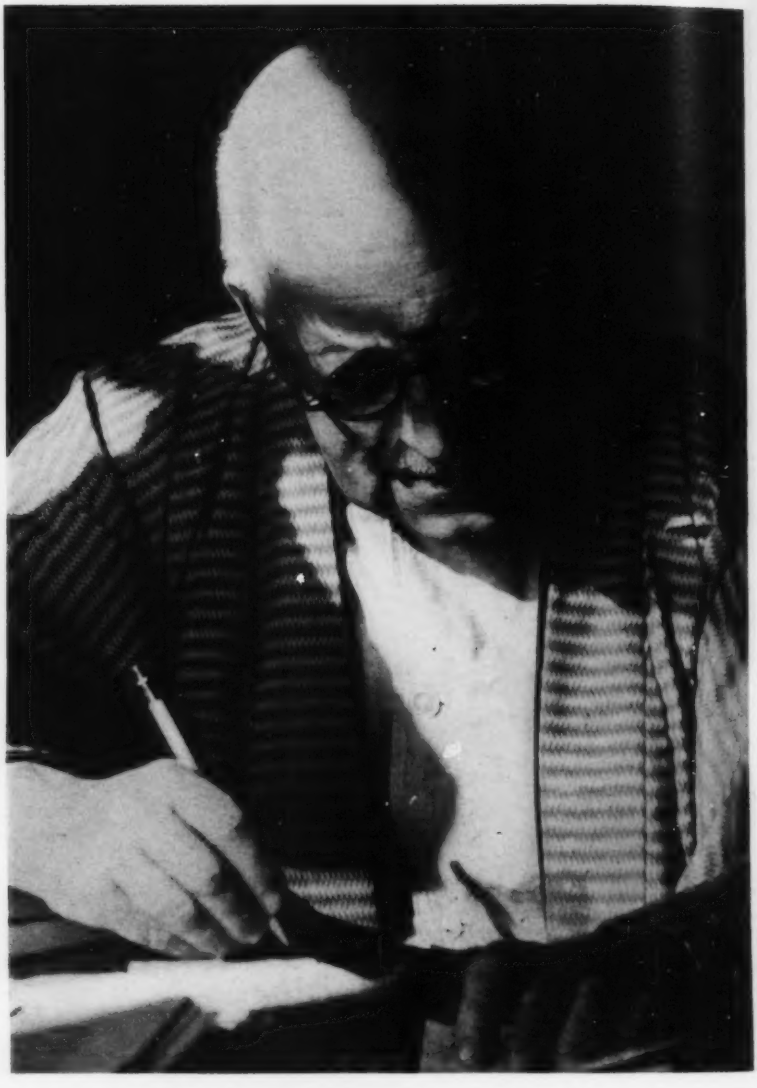

Fig. 3.

ARNOLD C. KLEBS, 1936.

born in Berne, that rare old city that has preserved its charm through the centuries. You grew up in a house where Science was worshipped, in Prague and in Zurich on the shores of a lake that is so unspectacular and so full of poetry:

Auf der Welle blinken
Tausend schwebende Sterne,
Weiche Nebel trinken
Rings die türmende Ferne;
Morgenwind umflügelt
Die beschattete Bucht
Und im See bespiegelt
Sich die reifende Frucht.

It was your privilege to study medicine in a great period under famous teachers in some of the most active medical centers, the Universities of Zurich, Berlin, Kiel, Würzburg, Berne, and Basel. You qualified as a physician in 1894 and continued your studies as a post-graduate student in Switzerland, London and Paris.

And then at the age of twenty-six you came to the United States that was to become your country of adoption. You are thoroughly Americanized, at least on the surface, but I think that at the bottom of your heart you still carry the mental unrest of the European as we intellectual hybrids of two continents all do. You went to Alabama first, then to Chicago. I do not know how gay the nineties actually were, but I know that to you they were years of hard work building up a practice as specialist and consultant in tuberculosis. And I also know that they brought a deep sorrow upon you, the death of your first wife. You grew up in an atmosphere of medical research and it was obvious that you would contribute your share. Your bibliography lists 31 papers and articles on various aspects of tuberculosis and in 1909 you edited the *American Treatise on Tuberculosis*.

This all was a record sufficient to secure for you a respected position in the medical fraternity. But then in 1909 a new period of your life began. You became increasingly interested in the history of medicine. I think I am not wrong when I say that William Osler and Karl Sudhoff were the strongest influences on this development

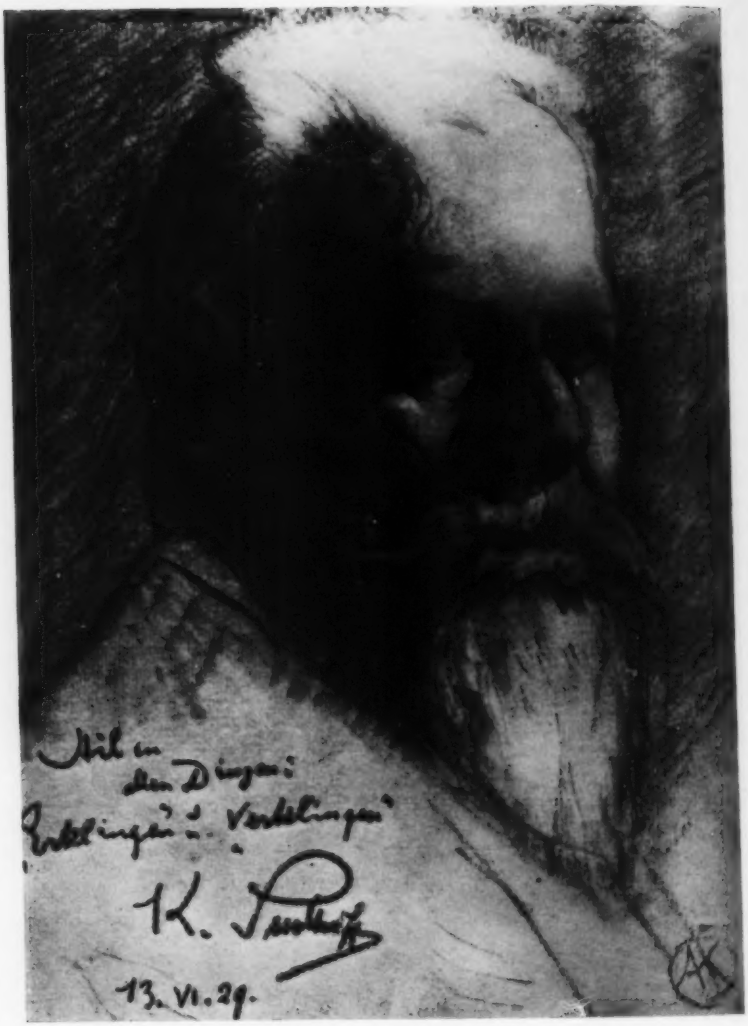

Fig. 4.

KARL SUDHOFF

Drawing by Arnold C. Klebs, 1929.

I notice that your first historical paper was "Osler at the tomb of Louis," while the second, the delightful sketch on the "Lemgoer Ratsapotheke" was published in one of Sudhoff's journals. From Osler you obtained the philosophic outlook and the faculty of presenting a subject with charm, and from Sudhoff you learned methods of scholarship.

Two topics attracted your attention particularly. One was the inoculation of smallpox, the literature of which you collected and described, and the history of which you wrote. The other was the Renaissance. An artist yourself and a frequent traveller to Italy, you felt at home in that period, the spirit of which you evoked in your papers on Leonardo da Vinci and Fracastoro. During the years of the war spent mostly at the Surgeon General's Library in Washington, you explored the medical and scientific literature of the 15th century, and you soon became an expert of world reputation on incunabula. Numerous were your contributions to the subject, particularly to the literature of herbals whose illustrations appealed to your artistic sense, and to the literature of the plague where you could combine your historical, bibliographical and epidemiological interests. And finally in 1938 you published your short title list *Incunabula Scientifica et Medica* that will remain a rich source of information and a standard book of reference like Choulant's bibliographies.

Medical history to you was never a dead subject, bibliography never an end in itself. You always felt that the history of medicine can and must become a living force and a driving power in modern medicine. I began my studies as a philologist but you know that today I share your views entirely and that I am trying to put them into practice.

Life gave you much more than intellectual curiosity and the faculty to pursue researches. Women were good to you and you enjoyed the rare friendship of such men as Osler and Cushing, to mention only a few. In Nyon, on the Lake of Geneva, you found a haven and you have a beautiful home whose hospitality has become proverbial. Mrs. Klebs, your wife, to whom we are very much attached has made it a paradise. You have a unique collection of books in which you have assembled the tools for research. You keep

Fig. 5.

ARNOLD C. KLEBS, 1938.

them in an enchanting "Häusel" to which you can retire and in which you can be "tutto tuo."

Your work is not finished. You know 15th century medicine better than anybody else. You must complete your magnum opus by adding to the bibliography the history of that literature. I beseech you to continue your work and not to let loose until you have rounded it up. I know that it is not easy. Your old friends have left you one after another. First it was Osler, then Welch, Sudhoff and recently Cushing and Jacobs. It is hard not to feel lonely. But you have other friends who believe in you. Among them are those who are paying tribute to you today in this number of the Bulletin of the History of Medicine.

Since we can not be with you physically to share your birthday cake we want to express our most cordial wishes in literary form. You will miss many names. This number was to be twice as large but the war and the disruption of postal services interfered. Some European contributors were called to arms and others could not let me have their manuscript in time. I wanted your father to be represented in this birthday number and so I reprinted one of his classical papers. The bibliography of your work was prepared by your faithful co-worker Mlle. Annie Lang.

Please accept this collection of essays as a token of friendship, devotion and admiration of some of your friends and particularly of

Your

HENRY E. SIGERIST

The Johns Hopkins University,
Institute of the History of Medicine,
March 17, 1940.

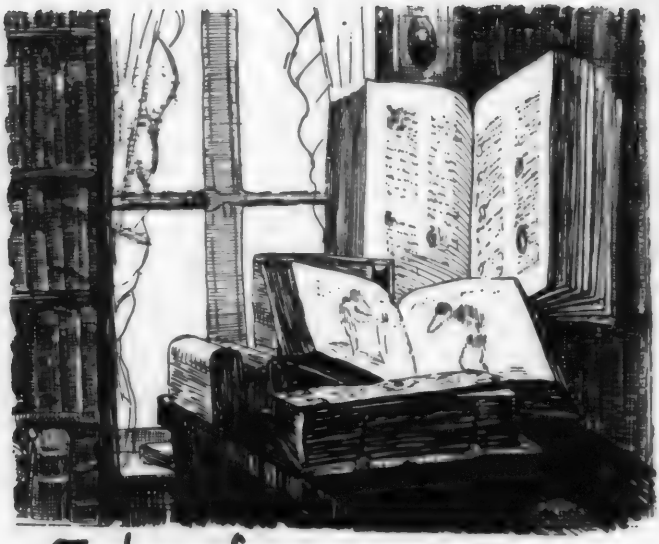
To be present as it were in
every age, to extend and
stretch life backward from
the womb, and thus extort
from unwilling fate a certain
forgone immortality.
Milton

Fig. 6.

Drawing by Arnold C. Klebs, 1929.

Courtesy of Dr. John F. Fulton.

Fig. 1.

ARNOLD C. KLEBS

in his study

1939.

Courtesy of Dr. John F. Fulton

DR. A. C. KLEBS AT SEVENTY

DR. B. W. TH. NUYENS
Amsterdam, Holland

In Nyon, between Geneva and Lausanne, two centres of study and science, there is a country house on the borders of the beautiful blue lake. It is called " Les Terrasses " and it is the home of Dr. A. C. Klebs, who will be seventy on 17th March, St. Patrick's day.

To those who know Klebs it is open to some doubt whether it will gratify him to find that his friends are not forgetting this occasion, sending him their congratulations and good wishes from all quarters of the globe; he is not the kind of man that likes to be fêted. But his friends think differently; it affords them great pleasure to be able to tell him and his charming wife of their joy at his still being so vigorous in mind and young of heart and they wish to give expression to that pleasure by offering him a collection of essays in which they dwell on the merits of his scientific work without offending his modesty.

The present small contribution from a friend in Holland has been written with the object of calling attention to Klebs's excellent work as a bibliographer and in promoting the study of books as such.

I deliberately mention the name of his home " Les Terrasses," because Klebs has founded in this peaceful and beautiful country a third centre of study and investigation, holding an honourable position beside Lausanne and Geneva. I must make it clear what I mean by this remark. In the grounds of " Les Terrasses " stands a square building, with no outward pretentions. It is Klebs's library, his workroom and at the same time a laboratory for those who wish to do bibliographical research work. Inside you will see nothing but books and writing tables and wherever there is a space of the wall unoccupied there are photographs of friends or visitors who worked there, finding every aid to their study that a bibliographer may require. It is a rich storehouse of books of a particular quality, but all the same a working library, for Klebs is no bibliophile, collecting incunabula, rare volumes or beautiful bindings for their own sakes.

Though he possesses a collection of works on variolation, inoculation and vaccination and a remarkable collection of books on tuberculosis, his main collection is the library of a bibliographer who has got together rich resources for his particular branch of study.

It was in these tranquil surroundings that Klebs acquired his knowledge of incunabula, becoming a recognized authority in this field. After years of preparatory work he there completed his "Short Title List of Incunabula Scientifica et Medica." This book, to all appearances a dull list of titles, is the fruit of steady toil and wide knowledge applied with the greatest care. It is a book that carries no particular appeal to the uninitiated, but to the student of books, the bibliographer, it is of the greatest interest.

To Klebs incunabula are not the rare volumes that excite the curiosity of the collector, nor the remarkable products of primitive presses of itinerant printers, but the literary documents of thinkers and seekers. The art of printing, which disseminated knowledge more rapidly and clearly than the older manuscripts, is the greatest invention of the fifteenth century. It especially promoted the development of natural science and medicine, which were already centuries old, but practised by few. For scientific workers and collectors and for those who cannot hope to see the completion of the "Gesamtkatalog der Wiegendrucke," Klebs's book is a reliable guide and a rich mine of information.

The writer of this contribution, who has often had the privilege of entering Klebs's workroom; who often had occasion to search for and find information in the rich library of "Les Terrasses," looks back gratefully towards this tranquil home of study and the remarkable man who made it into a centre of science. For Klebs is an out-of-the-ordinary, even somewhat singular, personality. By his extensive knowledge of the current living languages, he has made it easy for the foreign student to visit him. His visitors were invariably struck by his inner simplicity in an environment of luxury. Sometimes he appears to be a disciple of Zeno. I shall never forget his once saying to me, "I can live in a hut," and I am sure he meant it. He does not care for ideological speculation, but much appreciates an original idea. I fancy that he has a greater admiration for the discovery of electricity than for the invention of electric

light. All the same he is glad not to have to read by candle-light, even though he does sometimes resemble a Stoic philosopher. Klebs is no "laudator temporis acti," but has sufficient historical sense and common sense to be able to appreciate what is beautiful and good in former ages. It seems to me that had he lived in Nyon in the eighteenth century, he would have been an admiring neighbour and welcome guest of Mme de Stäel, Necker's famous daughter, the brilliant French esprit of the Napoleonic period, who received her circle in her home in Coppet. He would certainly have contributed his share to the conversation. It would have been just as interesting a contact if he had met the no less celebrated Jean Jacques Rousseau on one of the latter's visits to his father, who lived in Nyon. Klebs would have been quite frank in his criticism of, or agreement with, Rousseau's progressive ideas.

I do not imagine that in these lines I have been able to typify the man Klebs. My object was to claim your attention for the remarkable master of "Les Terrasses"; to show how usefully he has lived for seventy years; to express the hope that his brilliant mind may continue unimpaired for many years to come.

Amsterdam, in a crazed period of the world's history.

ARNOLD KLEBS AND HARVEY CUSHING AT THE 1ST INTERNATIONAL NEUROLOGICAL CONGRESS AT BERNE IN 1931 *

J. F. FULTON, M. D.
New Haven, Conn.

I

International gatherings of scientific men often have an importance which far transcends that of the formal reports of the meeting itself; and this was notably true of the Neurological Congress held at Berne in the autumn of 1931. There had previously been other international congresses of physicians, and even of neurologists, but to neurology, and to medicine generally, the meeting at Berne had peculiar vitality and significance. It was the first time after the World War of 1914-18 that neurologists from Germany, France and England, as well as of other countries of the world, had found it possible to have a joint meeting, and it proved to be one that was little marred by politics or the old animosities of war. But the gathering was also remarkable for other and quite different reasons; these had to do with personalities. In the first place the meeting was held at Berne, the birthplace of Arnold Klebs (Fig. 1), and there were present many of his oldest and most intimate friends: William Welch, Harvey Cushing, Charles Sherrington, Otfrid Foerster and many others, including Bernard Sachs, the President of the Congress, Hugh Patrick an old friend of Klebs's Chicago days, and Llewellys Barker whom he had known at the Hopkins.

On the opening day of the Congress, Arnold Klebs, in the true spirit of Swiss hospitality, gave a dinner [1] to which he invited many old friends, and a good many new and younger friends to meet one another, and also to meet his Swiss colleagues, Professors Sahli, de Quervain, Wegelin and Asher. The seating was as follows:

* From the Laboratory of Physiology, Yale University School of Medicine.

[1] Dr. Klebs had planned the dinner with the utmost care—truly Halstedian—and had had reprinted for the occasion Lücke's *Der Mohr von Bern* (1868), a volume which caricatured all the prominent members of the Berne medical faculty of 1868, including Edwin Klebs.

Fig. 1.

Arnold Klebs on terrace of Hotel Bellevue,

Berne, September 2, 1931.

(All photographs taken by Richard U. Light)

Dr. Ramsay Hunt
Dr. Gaston De Coppet
Dr. Ernest Sachs
Mr. Geoffrey Jefferson, F.R.C.S.
Dr. B. W. Th. Nuyens
Dr. Walter Timme
Dr. Reymond de Saussure
Prof. Vittorio Putti
Prof. F. de Quervain
Prof. Harvey Cushing
Sir Charles Sherrington
Prof. Otfrid Foerster
Prof. Leon Asher
Prof. Llewellys F. Barker
Dr. Charles Dubois
Dr. Foster Kennedy
Dr. Ign. Oljenick
Prof. Louis Michaud
Prof. Hugh Patrick
Mr. Norman Dott, F.R.C.S.
Dr. C. P. Symonds
Dr. R. H. Meagher
Dr. Henry Alsop Riley
Prof. Otto Marburg
Prof. John F. Fulton
Prof. B. Brouwer
[Prof. Hermann Sahli]
Mr. Hugh Wilson, U.S. Minister
Prof. William H. Welch
Dr. Arnold C. Klebs
Prof. Bernard Sachs
Prof. A. von Eiselsberg
Prof. Carl Wegelin
Dr. Percival Bailey
Dr. R. U. Light
Mr. George Armitage, F.R.C.S.
Dr. Franc Ingraham
Mr. Hugh Cairns, F.R.C.S.
Dr. Daniel O'Brien

At the end of the dinner Dr. Klebs, to whom I like to refer as "A.C.K.," made a speech of welcome, which no one present, dinner or no dinner, could possibly forget. I stole the text, copied it, and I give it to you here without securing the permission of the speaker. It began as follows without formal salutation:

"It is a singular privilege to welcome you, friends from across the sea, to Berne which I call my home-town, not only because of the accident of birth in it, but because of many precious bonds past and present.

"Welcome to you, Dr. Sachs, and thanks that you have brought together from many corners of the earth busy and distinguished workers, who, while they know each other well on paper, yet yearn for the living and warm contact which allows them to return to their task with renewed hope and vigor. It has always seemed to me that the real value of congresses lies in these personal contacts fully as much as in the actual business. Scientific thought, Dewey says, and I agree, is a function of society as well as of the individual, just as breathing is a function of the air as well as of the lungs.

"I am happy that my friends from the present medical faculty, Prof. Sahli, de Quervain, Wegelin and Asher, have joined me here to-night to add their welcome to mine. I present to you also their earliest predecessors in a reprint of Lücke's *Der Mohr von Bern*, the original of which has become very rare. It shows them in the lighter vein which apparently did not mar their historical record.

"Welcome to you, Patrick, friend of the old days in Chicago, where the

winds are cold sometimes, but hearts ever warm. Welcome to you, Bailey, who brought thither the spirit of the Brigham. Welcome to you, Dr. Barker, though you forsook early the Western Metropolis for Johns Hopkins.

"Johns Hopkins, what precious memories arise out of that name! Osler and Halsted, now gone but unforgetable. When I came first into contact with it my one desire was to become a student again. I had passed through seven universities but here it seemed I might really learn something. I had had splendid teachers before and the best equipment Europe could offer, but I missed what I found, and now can sum up in that inadequate expression 'American spirit.' At Johns Hopkins it was first infused into scientific medicine and from there it spread. We have with us tonight the man who knows better than anybody else how this miraculous infusion is performed. I am afraid he will not tell. President Hoover called him our greatest statesman of public health, but those who have benefitted by his fatherly counsel call him by a less formal but sincerely affectionate title. Dr. Welch, you are at home here as everywhere, we welcome you most cordially.

"In matters of the 'American spirit' I must read you what a recent Dutch critic (Huizinga) has said about it: 'Es besteht in Amerika ein geistiger Zusammenhang, ein gemeinschaftliches Streben, wie *wir* es nicht kennen . . . Männer und Frauen der Wissenschaft arbeiten als Alle opfern sie etwas von ihrer eigenen Persönlichkeit . . . Sie verzichten wenn auch unbewusst, auf die Luftschlösser Ihres eigenen Denkens, um für die andern zu pflügen . . .' Let us keep this precious gift!

"And now, dear Harvey, what shall I say to welcome you to the town that is as much yours as mine? The letter you wrote home from here in 1901 gives me the key. Your versatility has always amazed me but I did not realize what an excellent medical historian you were already then. The preoccupation with batrachian hearts at Kronecker's and with thyroid complexities at Kocher's did not prevent your going to the Stadtbibliothek to study Haller at the source, in his *Tagebücher*. His unfathomable erudition bothered you and you wanted to discover his human side. It relieved you to find him noting 'perdu au jeu' or a 'bouteille de vin.' You compared him to John Hunter who hated lecturing, but became the guiding star of many followers, while Haller with all his gifts left no personal school. You in your own career have followed Hunter. Your boys from Brigham, some of whom I am delighted to see here with you, will heartily agree that you have succeeded. They are your living history. Some historians can predict but to few it is given to show that they have learned from history. Keep it up, Harvey, give us some more 'from tallow-dip to television.' Don't forget what you said in perfect Bärndütsch: Nit nolo g'wunnt!"

Dr. Welch replied for the guests—with his usual felicity and charm—and he characterized A.C.K. as one who had "the rare gift of friendship." He also spoke of the remarkable letters which

all his friends enjoyed and treasured, and of his great bibliographical erudition. No one but " Popsy " could have expressed so effectively what we had all felt in our hearts.

II

But we must now retrace our steps. The 1st International Neurological Congress was held at the Municipal Casino, beginning on Monday morning August 31st. A surprise had been arranged by Professor Asher, then " Rector Magnificus " of the University, in the form of two honorary degrees, one for Harvey Cushing and the other for Charles Sherrington. Neither recipient had been previously informed of the honour, and it had required some little diplomacy to ensure their presence at the meeting without revealing the reason for having them prompt in their time of arrival. Sherrington, as one of the Vice-Presidents of the Congress, sat on the platform. With some difficulty Dr. Cushing was persuaded to sit in one of the front rows. Beside him were William Welch and Arnold Klebs.

The meeting was opened at nine sharp by Herr Haeberlin, the President of the Swiss Federation, who welcomed the delegates in the name of his democratic country. He spoke in a clear, slow French, most happily and to the point. Then came addresses of welcome by four Swiss Professors, Asher, Wegelin, Naville and Bing, each of whom spoke in a different language (English, German, French and Italian). The Rector Magnificus then took the platform, referring first to Pavlov, which called forth an enthusiastic response. He then announced that the University was to do honour to itself by the conferring of two honorary degrees, the first to Harvey Cushing. He then called for H.C., who walked slowly up the steps on the left side of the rostrum and crossed to the opposite side to face Asher. A brief statement was then read in Latin in which H.C.'s achievements were described. Dr. Cushing meanwhile stood facing Professor Asher, profile to the audience, without moving an eyelash and his face, although almost immobile, gave evidence of suppressed pleasure and emotion. After all this, Asher turned to him, grasped his hand, and spoke spiritedly and in perfect English somewhat as follows:

" Harvey Cushing, when you first came to Berne, thirty-one years ago,

Hugo Kronecker and I saw in you the future master of neurological surgery. You have more than lived up to our expectations, for in your field you have no peer. We are proud to think that some at least of your inspiration came from the Laboratory of Physiology at Berne and we are even more proud now to welcome you as a member of our University."

Asher also referred to H.C.'s relations with Kocher and to all of his many friends of early days, male and female, who recalled him vividly and were proud to have known him. The diploma in a large red case was then presented to him.

Professor Asher next called upon Sir Charles Sherrington who, like Cushing, was quite dumbfounded for the moment; and repeated much the same performance first in Latin, followed by a speech in perfect English vernacular. His references to Sir Charles were particularly happy and were terminated by the phrase: " Sir Charles, we look upon you as the supreme philosopher of the nervous system."

The opening proceedings were terminated by Bernard Sachs's excellent Presidential Address. His references to Switzerland as the home of human liberty seemed especially well timed. After the formal part of the morning session the scientific program began with a number of important papers by Clovis Vincent, Percival Bailey, Wilder Penfield and several others.

On Monday afternoon the 31st, Dr. Cushing read, as the third speaker on the scientific program, his celebrated report on 2,000 verified tumours of the brain. At least a thousand people attended, and for the first time (and probably the only session during the Congress), all of the side foyers were empty. Dr. Cushing, speaking slowly into a microphone which seemed perfectly tuned to his voice, began with references to his early physiological experiments in Berne carried out in 1900-1901, mentioning that he had now come to give an account of the work he had done in the interval. He referred to the various factors which had led to the dramatic fall in mortality rate for cerebral operations. " Younger men," he went on to say, " picking up where I leave off, can reduce the mortality still further." He reviewed the life history of the various categories of brain tumour and then ended with a somewhat unexpected climax: " Gentlemen, this will be the last report on the statistical results of brain tumours as a whole which I shall ever publish." [2]

[2] Dr. Cushing's full report was later published in monograph form under the

For a moment there was complete silence, then a burst of prolonged applause. Professor Ariëns Kappers, who was presiding, broke the precedent by offering a vote of thanks to Dr. Cushing in the name of the Congress for placing before them in this inspiring way the brilliant results of his life's work.

On Monday evening, Arnold Klebs gave the dinner for Dr. Cushing and his pupils to which I referred at the beginning of this account.

Tuesday, September 1st

The following day began with a morning session devoted to muscle tone and the postural reflexes, with Sherrington in the chair. There were many papers. Little new, however, was brought forward and the session was chiefly remarkable for the nationalities represented and for the brilliant summing-up at the end by Sherrington. He expressed his objection to the word 'tonus' itself and voiced the hope that the term would ultimately be dropped; so long as it is employed an element of mystery will hang about it and since we now know that it represents postural contraction, it were better to use this latter term.

In the afternoon there were demonstrations and no end of fascinating anecdotes. Dr. Welch, for example, told us of his student days in Vienna, Berlin and Breslau (1876-1877). He knew Rokitansky and had informed Cohnheim how to stain tissues with haematoxylin and eosin; at Ludwig's laboratory he discovered the bipolar ganglia in the frog's heart, but Ludwig had not encouraged him to publish the observations. He knew Roy and attempted with him to establish an Anglo-American journal of experimental pathology. There seemed no end during these days of fascinating reminiscences, but H.C., even more than Welch, formed the centre of attention. He was besieged by his old friends, many of them by now dignified matrons, and also his lodginghousekeepers of 1900 and 1901, booksellers, reporters and foreign physicians.

A wealthy family made a pilgrimage from Hungary with a child that had an odd-shaped head. They camped on his doorstep until

title: *Intracranial tumours. Notes upon a series of two thousand verified cases with surgical-mortality percentages pertaining thereto.* Springfield, Ill.; Charles C Thomas, 1932. 8vo, xii, 150 pp.

Fig. 2

Dr. Cushing, his teachers and his pupils. September 1st, 1931. From left to right, *Above*: Drs. Armitage, Petit-Dutailles, Bremer, Martin, Oljenick, Bailey, Penfield, Schreiber, Bagdazar, Olivecrona, Morelle, Schaltenbrand, Ingraham; *Middle*: Drs. Francis Grant, Arnold Klebs, William Welch, Harvey Cushing, Charles Sherrington, Thierry de Martel, Geoffrey Jefferson, Tracy Putnam; *Below*: Drs. De Coppet, Meagher, Fulton, Dott, Cairns, Fremont-Smith.

he had to see them. As the baby was the son and heir he was never allowed out of his mother's sight. H.C. sized up the situation in a flash, saying: "You need not come to Boston; you have only to let your child run and play with other children." Whereupon he took the child down the hotel corridor, making him chase a ball. The mother, the sisters, the cousins and aunts remained in the room aghast and wide-eyed; presently the docile infant crept back and startled them all with a vociferous 'Boo!' In five minutes H.C. had thus cured the family as well as the child. The astonished but obviously delighted father enquired what his fee would be; H.C. said there would be none, whereupon the father protested. H.C. then asked, "Do you know Jeremiah Smith?" Their faces all brightened. They had known him well. "He is one of my best friends," said H.C. "I feel towards Hungary as he does." At this they all made a low bow and retired.

Dr. Cushing's Dinner [3]

"On the spur of the moment this morning, H.C. decided to invite all of his former pupils to dinner to meet his 'masters,' Welch, Sherrington, de Martel and Klebs. I had to dash around and find them in the course of the morning and everyone accepted—28 in all—and it proved a memorable occasion. The seating, which H.C. slaved over for nearly two hours (writing all the cards himself), was as follows (Fig. 2):

H.C.

Thierry de Martel (Paris)	Dimitri Bagdazar (Bucharest)
Paul Martin (Brussels)	Percival Bailey (Chicago)
Daniel Petit-Dutailles (Paris)	Georges Schaltenbrand (Hamburg)
Tracy Putnam (Boston)	Frederic Schreiber (Detroit)
Jean Morelle (Louvain)	Richard Light (Boston)
John Fulton (New Haven)	Herbert Olivecrona (Stockholm)
Dr. Welch (Baltimore)	Dr. Klebs (Nyon)
Richard Meagher (Boston)	Hugh Cairns (London)
Francis Grant (Philadelphia)	Ignaz Oljenick (Amsterdam)
Norman Dott (Edinburgh)	George Armitage (Leeds)
Frank Fremont-Smith (Boston)	Gaston DeCoppet (Berne)
Frédéric Bremer (Brussels)	Franc Ingraham (Boston)
Sir Charles Sherrington (Oxford)	Geoffrey Jefferson (Manchester)
	Otfrid Foerster (Breslau)

Wilder Penfield (Montreal)

[3] From a diary kept during the Congress.

"I had the good fortune to sit beside 'Popsy,' who remarked in a quiet way that the gathering had a far-reaching significance since it meant that someone in America had at last founded a 'school,' and that the 'school' was exerting an influence perhaps even greater abroad than at home. The followers of Cushing were fired with much the same enthusiasm that had fired the students of Ludwig nearly a century before. There was much in this vein and many reminiscences. 'Popsy' never repeats himself and he has never told a dull anecdote.

"As the dinner was drawing to an end, 'Popsy' suggested that I propose the health of 'The Chief'—who had meanwhile moved to the end of the table, exchanging places with Penfield. His response was moving, witty and very simple, and began as most of his speeches do, with a humorous twist:

Child: 'Papa, where were you born?'
Papa: 'In Berlin.'
Child: 'Mama, where were *you* born?'
Mama: 'In Paris, my dear.'
Child: 'Where was I born?'
Mama: 'In Baltimore. Don't you remember?'
Child: 'Isn't it funny how we all got together!'

Then he went on somewhat as follows:

"Everything has a small beginning. Henry Head once said that there would have been no Great War had he not once looked through a peep-hole in a girl's school. We could not be sitting here had a Baltimore saloon-keeper not shot his wife in the neck in a moment of extreme emotion in September 1896. She by chance was a patient of mine and had haemato-myelia with some of her dermatomes knocked out. This led me to Kocher's book and I decided to come to work under him. In 1900 I sailed with the desire to work with three people: the man at my left [Sherrington, Fig. 3], which explains his being here; with Horsley, who unhappily has joined his fathers; and with Kocher of Berne. The only note of introduction was a letter to Horsley which Dr. Welch (Fig. 3) had given me the night before I left. Then as now, Welch spent his life seeing how he can make the way easy for other people [burst of applause]. Horsley was delightful, but was running for Parliament and I decided to return when he had either gotten in or gotten out! I went to Paris and then to Berne, but waited a month for Kocher to give me an *Arbeit*. Then I found myself in Kronecker's laboratory. This brief story accounts for everyone here except Jefferson, de Martel and Olivecrona, who speak for themselves. So it is natural that we should foregather here in Berne. I hope we shall go out to see De Coppet operate on Friday; if things go well, we shall rejoice with him; if not, we shall all know that each of us has done a much worse job himself.

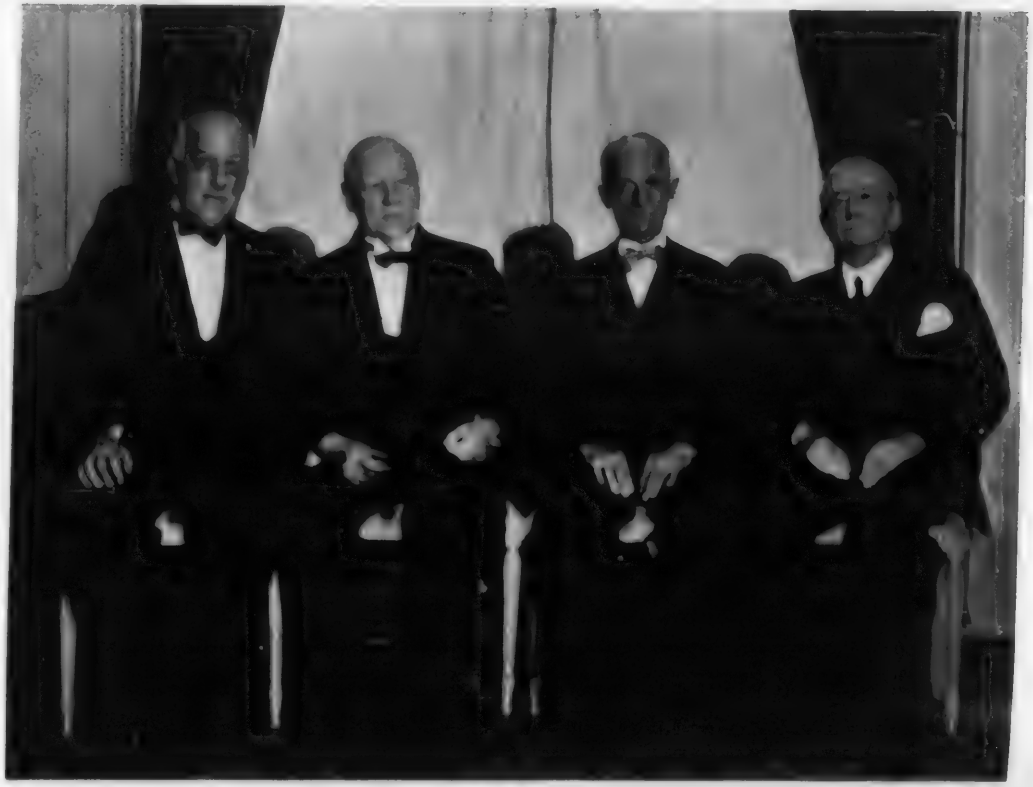

Fig. 3.

Arnold Klebs, William Welch, Harvey Cushing, and Charles Sherrington, September 1st, 1931

"Funny how we all got together!

"Hold together. Keep up your friendships—form societies where you can meet together frequently to exchange ideas, and to criticize one another's technique. Keep your finger on neurology, but don't get lost in scientific minutiae—a medulloblastoma or a perineurial fibroblastoma by any other name is just as sweet! Don't let neurological surgery get too far away from general surgery. It is the greatest possible compliment that we have been adopted by the neurologists and accepted as one of them. Be this as it may, we did not grow wholly out of neurology, for our roots are in the fertile soil of general surgery. I like to think that our specialty is perhaps the richest in the field of Medicine, and it will be if you make it so."

Thursday, September 3rd

Wednesday, the 2nd, had been devoted to excursions and nothing worthy of special record took place except that a good many of us were glad of the opportunity to recover from the excitement of the previous two days. On Thursday, at noon, after the morning session, a group of some thirty people went out to the Berne Cemetery for an unforgetable ceremony.

III

The Laying of the Wreaths

In the pouring rain, Welch, Sherrington, Cushing and Arnold Klebs, along with several Swiss physicians (de Quervain, De Coppet and Fischer) went out with a number of the younger American physicians to lay wreaths on the graves of Kocher, Kronecker and Edwin Klebs. The ceremony resulted from Dr. Cushing's inspiration and nothing could have been more touching to the hearts of the people at Berne. The group first went to the tomb of Theodor Kocher which lies at the far end of the cemetery. The rain continued and H.C., picking up a large wreath went slowly up to the tablet and laid it at its foot. He came back to the plot and spoke for about ten minutes, coatless, his head bared (Fig. 4) and the rain soaking him, as follows:

Dr. Cushing on Kocher

"In my younger days I had the good fortune to come under the influence of two men who were outstanding surgeons of their generation—William S.

Fig. 4.

Dr. Cushing speaking beside the monument of Theodor Kocher. September 3rd, 1931.

Halsted and Theodor Kocher. I may couple their names together for they had much in common. They were engaged in similar problems; they were friends and correspondents; they were equally fastidious in their operative craftsmanship and at the same time, by precept and example, exerted a profound influence as investigators on the scientific aspects of their art. They represented the European and American surgery of their day at its very best.

"So in placing this wreath, in loving tribute, on the grave of one of these masters, I feel that I am merely the agent of the other, his admiring friend, whose memory I hold in equal affection and gratitude.

"The most precious heritage of our profession lies in its noble traditions. What has been accomplished does not die, but too often, alas, the personality of those who have handed the torch from one generation to another soon fades into oblivion. So for those of you—his spiritual grandchildren—who have gathered here and to whom Theodor Kocher is little more than the name of a street which you have frequently traversed the past few days, I would like to give at least an impression of what he was in life—a slight, spare man of personal neatness, of quick step and alert bearing, of unfailing courtesy and dignity, precise and scrupulous in all his dealings professional, public and personal—a man to trust.

"His clinic at the Inselspital had long been a surgical Mecca and when in 1900 I came here hoping to find an opportunity to work under his guidance he finally gave me a problem which took me away from the clinic to the physiological laboratory. When sometime later on he chided me for my failure longer to appear in his operating room I awkwardly replied that after witnessing a few of them, to learn his methods, I found his operations singularly uninteresting; but I hastened to add that for this very reason I would unhesitatingly and with perfect confidence put myself in his hands, should I at any time happen to be in need of an operation myself. For a moment he looked mystified and then said: 'I shall interpret your absence then as a compliment. Surgeons who take unnecessary risks and operate by the clock are exciting from the onlookers' standpoint but they are not necessarily those in whose hands you would by preference choose to place yourself.'

"We think of America as the land where young men are given their chance; but in 1872, at the unusual age of 30, Kocher succeeded Lücke in the Chair of Surgery here. And here to the end, a loyal Berner Burgher, he remained in spite of tempting calls to posts in other universities possibly of greater renown. It is given to few surgeons continuously and actively with ever increasing reputation to hold a professional chair for so long a time as forty-five years. I last saw him on his seventieth birthday when in 1912, shortly after he had been made a Nobel laureate for his contributions to our knowledge of the thyroid gland, a *Jubeleum* was held here in his honour and at this great festival tributes from every part of the world poured in upon him. From hard work and responsibility surgeons are prone to burn themselves out comparatively young, but Kocher had been blessed with an imper-

turbability of spirit or had cultivated these habits of self-control which enabled him to bear his professional labours, his years, and his honours with equal composure to the very end. The current of his long and active life was as steady, cool and uninterrupted as that of the Aare encircling his beloved Berne."

Hugo Kronecker. From Kocher's tomb we came slowly to Kronecker's and that of Edwin Klebs, which are together in an enclosure. Sir Charles laid the wreath on Kronecker's monument and reminisced most felicitously about him (Fig. 5). Just as he started, however, there was a loud clap of thunder which caused him to look quizzically at the sky as if annoyed with the Almighty. He told the story of Kronecker's habit of being late and of Holmgren's celebrated rémark when awaiting him at a committee: "Der kronische Verspätung!" Sir Charles spoke as follows:

SHERRINGTON ON KRONECKER

"We, coming, some of us from homes far distant, offer here together our token of respect and affection to memories intimately with us in this place. Our thoughts turn to three names which, in their time—the generation immediately preceding our own,—contributed to adorn Berne as a source of medical culture; the names of three men who lived to ennoble medicine both as art and science by their work and their example. Of them at this spot where we now stand lie two; and for one of them, Hugo Kronecker, the tributary wreath we bring to him, now rests against his grave.

"His name was early familiar to the young physiologists of my generation because so often was it on the lips of our teachers; and always with a note of special warmth of admiration. Later I had, through many good years, the privilege and pleasure of personal contact with him. So it came that I experienced myself the charm of his gifts and character. To know him was to understand the universal regard in which he was held. A circumstance which favoured our friendship was the organizing of the International Congresses of Physiology. Those meetings could not have found a more devoted or unselfish collaborator than he. At Berne, in the 'Hallerianum,' as he loved to style the laboratory, he presided over the Congress of 1895; to its outstanding success his own personality contributed no small share.

"As for his services to his science, they were many. Research, in which the action of cardiac muscle was perhaps his favourite theme; apparatus, his ingenious instruments made his name a household word in many a laboratory outside his own; and thirdly, he typified a fine tradition of laboratory comradeship, and so genially and happily that he stood as its very embodiment for us all. He had a genius for friendship, with old and young alike—even

Fig. 5.

Sir Charles Sherrington speaking at the grave of Hugo Kronecker. September 3rd, 1931.

with children. I can still see him in a schoolboy's room, during a visit to England, standing on a chair fixing to the roomwall a clock brought with him from Berne and pieced together by him to delight the young friend with its jubilant 'cuckoo.' His silvered head will be remembered with affection not only by those many who recognized him as an honoured master of our science, but by many who, apart from all that, knew and loved him for his nature as a man."

Dr. Welch on Edwin Klebs

Dr. Welch, standing before the tomb of Edwin Klebs—utterly oblivious of the drenching rain spattering on his bald head—spoke for fifteen minutes extemporaneously of Klebs's life and his achievements (Fig. 6). No one, alas, was there to take it down and he had no notes—just the resources of a memory which seemed never to fail. He recalled Klebs's full name: Theodor Albrecht Edwin, and that he had been born 97 years earlier, *i. e.*, on February 6, 1834. "Popsy" described him as a restless man of profound prescience, whose many peregrinations entitled him to be called a "wandering scholar." For he had lived successively in Königsberg, Berne, Prague, Zürich, Karlsruhe, Asheville (North Carolina), Chicago, Hannover, Berlin, Lausanne; and finally he returned to Berne to spend his declining years. Of his achievements he mentioned the joint discovery with Loeffler of the diphtheria bacillus, his description of acromegaly in 1884, and that fifteen years before Metchnikoff's studies he had inoculated monkeys with the exudate of syphilitic lesions. Mention was also made of his introduction of solid culture media before Koch, and of his isolation, prior to Eberth, of typhoid bacilli from cases of typhoid fever.

Much else was said, but words left less of an impression than vivid memory of an occasion so unique—everyone was deeply moved, Arnold Klebs perhaps more than anyone else and certainly more than he ever confessed.

Otfrid Foerster who, during his rich and productive life, has always profoundly admired Kronecker, Kocher and Klebs, remarked: "Harvey Cushing was inspired to think of this, and to have been present was the greatest privilege I have ever had."

Fig. 6.

Dr. Welch speaking before the grave of Edwin Klebs. September 3rd, 1931.

IV

The Congress went on for another day. After the ceremony at the cemetery Professor and Mrs. Asher gave a memorable luncheon party, to which many went direct from the cemetery. There were present:

Dr. Graham Brown (Cardiff)	Dr. Pavlov (son of Prof. P.)
Dr. von Weizsäcker (Heidelberg)	Dr. Sachs (Pres. of Congress)
Prof. Pavlov (Leningrad)	Prof. Asher (Berne)
Frau Asher	Sir Chas. Sherrington (Oxford)
Dr. Cushing (Boston)	Dr. Welch (Baltimore)
Dr. Springer (Berlin)	J. F. F. (New Haven)

Pavlov and "Popsy" were the chief centres of attention. Dr. Welch took an active part in the conversation about journals—a topic which had been brought up by Dr. Ferdinand Springer of Berlin—and he told of his early experiences with the *Journal of Experimental Medicine*. They founded it in 1896 in order to give a channel of publication for experimental work in the broad field of the medical sciences [See his preface to the first volume]. In the early volumes he used English spelling entirely; later, spelling preferences were allowed; since it has been taken over by the Rockefeller Foundation the conventions adopted have been rather vigorously standardized, but the grotesque A. M. A. spelling conventions have not been adopted. The *American Journal of Physiology* was an offshoot of Dr. Welch's journal.

Pavlov's Paper [4]

"Pavlov had been a little uneasy during lunch as his paper was obviously on his mind. The session was held in the 'Aula' at the University. He was called on at 3:00 and the whole room was thronged; as soon as he mounted the platform they all stood and cheered for several minutes. As usual he looked pleased, but not in the least surprised! He spoke in German on neuroses in animals, briefly and quite effectively. Sir Charles, who has never seen quite eye-to-eye with Pavlov remarked afterwards: 'He has accumulated an enormous body of significant experimental data, but his attempts at interpreting it are infantile!!' I had had no idea that he felt so strongly.

"*The Hallerianum.* After Pavlov's paper Asher took me to his laboratory

[4] From a diary kept during the Congress.

which was built about 1890 by Kronecker and is known as the Hallerianum in honour of the great 18th Century physiologist. It is a yellow brick structure about the shape of the old Sterling Hall of Medicine at Yale, and it has been very little changed since it was erected. He took me into his office and showed me first the conventional engraving of Ludwig. 'That,' he said, 'was the Ludwig who taught Bowditch; and this,' pointing to a bust in profile over his desk, 'is the Ludwig who taught me!' And the spirit of Ludwig somehow permeates the laboratory, and it is very refreshing; Asher is almost the last of those who can call themselves Ludwig pupils. The laboratory is fairly well equipped with apparatus, but Asher talks poverty constantly; despite this a surprising amount of work is going on, and each year he has pupils collaborating with him from all over the world. He showed me with considerable emotion the corner room in which Dr. Cushing had worked in 1900-1901."

In the evening of September 3rd came the official dinner of the Congress with, alas, speakers from every country. H.C. and "Popsy" both spoke but unfortunately they were scarcely audible. Foerster, however, made a brief but most eloquent and audible plea for the recognition of Neurology as a separate discipline, and as far as formal utterances were concerned it was the high point of the entire Congress. Since it has not previously been published I venture to give it here in full.

Herr Präsident!
Meine Damen und Herrn!

Als vor 2 Jahren der Aufruf zur Teilnahme an den Internationalen Neurologen-Congress auch an Deutschland erging, da haben alle deutschen Neurologen diesem Rufe begeistert zugestimmt. Und wenn trotzdem heute die Gesellschaft Deutscher Nervenärzte leider nur durch eine verhältnismässig geringe Zahl ihrer Mitglieder hier vertreten ist, so wollen Sie darin bitte nicht einen Mangel an Interesse an diesem Congress erblicken, der durch die grosszügige Initiative der American Neurological Association inauguriert worden und bisher so glücklich verlaufen ist. Nicht Mangel an Interesse, nur die bittere Not der Zeit ist es, die so viele Deutsche Neurologen heute von hier ferne hält. Aber im Geiste weilen sie alle hier und mit den herzlichsten Wünschen begleiten sie den Verlauf dieses Congresses vom ersten bis zum letzten Augenblicke.

Und in der Tat! kann es denn für uns ein schöneres, ein erhabeneres Ziel geben, als wenn wir, die Erben eines Romberg, eines Leyden, eines Wilhelm Erb, Hermann Oppenheim, eines Hitzig, Flechsig, Wernicke, uns zusammenfinden mit den Erben des unsterblichen Hughlings

Jackson, mit den Erben eines Sir William Gowers, Sir Victor Horsley, Sir David Ferrier, Sir Byrom Bramwell, eines Bastian; als wenn wir zusammenkommen mit dem Mann, der wie ein Zauberkünstler in die geheimnisvollen Zusammenhänge des nervösen Geschehens hinein geleuchtet hat wie kein anderer, mit dessen Namen die Physiologie des Nervensystems steht und fällt, Sir Charles Sherrington, als uns zusammenzufinden mit den Erben eines Brown-Séquard, eines Duchenne de Boulogne, des grossen Charcot, eines Brissaud, eines Dejerine, eines Sicard, eines Foix, eines van Gehuchten, eines Wertheim-Salomonson, eines Bolk, eines Retzius, Salomon Eberhard Henschen, Carl Petrén, eines Ernst Alexander Homén, eines Korsakow, Kojewnikow, eines Roth, eines Jendrassik, eines Pilcz, eines Golgi, Mingazzini, Camillo Negro; als zusammenzukommen mit den Jüngern des greisen Titanen, der den Ariadnefaden, der in das Labyrinth des Nervensystems hineinführt und wieder daraus hinausführt, an silberner Spindel auf- und abgesponnen hat, Ramon y Cajal, als zusammenzukommen mit den Erben eines Dubois, eines Monakow, eines Forel, als einzuschlagen in die Bruderhand der Erben eines Türk, eines Meynert, eines Nothnagel, eines Heinrich Obersteiner, eines Emil Redlich, als uns zusammenzufinden mit den Erben der grossen Neurologen aus der Neuen Welt, eines Weir Mitchell, eines Dercum, eines Charles Mills, eines Sir William Osler. Und kann es für uns deutsche Neurologen überhaupt ein schöneres, ein erhabeneres Ziel geben, als dass auch wir, voll aufrichtigster Dankbarkeit und feuriger Inbrunst, zusammen mit allen anderen Neurologen, dem Manne begeistert zujubeln, welcher der prominenteste Eckpfeiler unserer Wissenschaft ist, dem unser Sonderfach, die Neurologie, überhaupt erst die Stellung einer eigenen selbständigen Disziplin verdankt, dem grossen Forscher, dem gesegneten Helfer der Menschheit, "der die Blinden sehend und die Lahmen gehend macht," der wundervollen Persönlichkeit Harvey Cushings!!!

Meine Damen und Herrn! Wenn wir uns hier zusammenfinden und wenn wir, vom Geiste dieser grossen Männer einen Hauch verspürend, hier in diesem gottbegnadeten Lande, der Schweiz, dem Lande, da einst der Freiheit Wiege stand, dem Lande, das Hort und Hüter höchster Kulturgüter stets war, noch ist, und immer sein wird, am Fusse der gewaltigen Bergriesen, die, vor Aeonen von Jahren erstanden, noch immer ihr schnee- und firnbedecktes Haupt zum Himmel emporheben, wenn wir uns hier gleichsam einen "Rütlischwur" einander in die Hand leisten, dass wir das Erbe, das wir von unseren Vätern übernommen haben, getreu bewahren und vermehren wollen und dass wir diese heilige Pflicht auch unseren Schülern und Nachfolgern ins Herz legen wollen, ich meine, dann erscheinen wir als Teile eines grossen Organismus, der ewigen Bestand hat, und dann erleben wir zugleich einen jener seltenen und grossen Augenblicke, in denen Zeit und Ewigkeit sich paart.

Meine Damen und Herrn! Die medizinische Wissenschaft gehört zu den Kulturgütern, die Gemeingut aller Nationen sind. Sie ist mit an erster Stelle dazu berufen, ein gemeinsames Band um alle Völker zu schlingen. Sie ist der Boden, auf welchem Angehörige der verschiedensten Rassen, mögen ihre Charaktere und Anschauungen auch noch so verschieden sein, mögen ihre sonstigen Ziele und Wünsche auch noch so weit divergieren, zu einer grossen Synergie zusammentreten. Es will mir scheinen, dass unter den Vertretern unseres Sonderfaches, der Neurologie, von jeher ein besonders gutes Einvernehmen geherrscht hat, ein besseres gegenseitiges Verstehen gewaltet hat als in anderen Sonderdisziplinen der Medizin. Ich glaube deshalb, weil das Fach der Neurologie von jeher ein besonders exaktes gewesen ist, weil die Forschung sich immer und überall auf den gleichen grossen Linien bewegt hat. Ich bin überzeugt, dass dies immer so bleiben wird, so lange die Neurologie den festen Boden der Tatsachen nicht unter den Füssen verliert, so lange die anatomische und die physiologisch-analytische Beobachtungsweise das Feld beherrscht, so lange die Vertreter unseres Faches sich nicht in die Gefilde der Mystik verlieren, nicht abschweifen in die nebligen Sphären einer nach freiem Belieben interpretierenden Symbolik, sich nicht an Worten einer oft schwer verständlichen Terminologie berauschen, uneingedenk eines der vielen tiefsinnigen Worte des grössten Neurologen aller Zeiten, Hughlings Jackson: "Words fetter our thoughts as well as define them."

Meine Damen und Herrn! Wenn wir einen Rückblick werfen auf den bisherigen Verlauf dieses Congresses, so dürfen wir wohl ohne Ueberhebung sagen: Dieser erste Internationale Neurologische Congress ist ein grandioser Erfolg! Er ist eine geradezu imposante Demonstration der grossen Bedeutung und des riesigen Umfanges, den die Neurologie heute erlangt hat. Aber wir haben nicht nur unsere Anschauungen und Kenntnisse über die wichtigsten schwebenden Probleme und Fragen gegenseitig ausgetauscht. Wir sind uns auch menschlich einander näher getreten, haben neue Bekanntschaften geschlossen und alte Freundschaften erneuert. Wir haben Männer, deren Werk und Namen uns seit langem bekannt, von Angesicht zu Angesicht geschaut, von Mund zu Ohr vernommen. Ich glaube das bedeutet viel. Befruchtend ist und segensvoll der Regenstrom, der aus der dunklen Wolke niedergeht, doch zieht der Wanderer, der nach der Wahrheit lechzt, den hellen Quell ihm vor, der aus des Felses scharf umrissener Spalte stürzt!

Dieser Congress, ich wiederhole es, ist ein beispielloser Erfolg. Trotz des schier babylonischen Sprachengewirrs, das hier herrscht, stehen wir doch alle hier, als ein einig Volk von Brüdern, zu unserer Alma Mater Neurologica! Möchte dies immer so bleiben! Hoffen wir, dass dieser erste Internationale Neurologische Congress bald von weiteren gefolgt sei, auf denen die Grundlagen, die hier gelegt wurden, weiter ausgebaut werden, zum Heile und Segen der Wissenschaft, der wir alle mit gleicher, heisser Liebe dienen!

Zum Augenblicke möcht ich sagen: "Verweile doch, du bist so schön!" "Dann wird die Spur von unsren Erdentagen nicht in Aeonen untergehn!"

V

And so the Neurological Congress of Berne drew to a close. Arnold Klebs, through his warm affection for Harvey Cushing and his unusual capacity for bringing people together, had made it an event with few parallels in the history of such gatherings. A new "school" in the ranks of Medicine had come suddenly to have international recognition; and the *esprit de corps* which this has fostered in the group itself has found substantial expression in countless ways [5]—but in none more poignantly than in the elaborate organization in all European countries now at war [6] for dealing with head injuries. In each country those responsible for these services have been trained by Dr. Cushing or by the pupils which they in turn have inspired; but Harvey Cushing's influence has touched nearly every sphere of modern Medicine.

[5] At the time of Dr. Cushing's 70th birthday six separate publications appeared in his honour. See the "Preface" of *Harvey Cushing's Seventieth Birthday Party, April 8, 1939.* Published for the Harvey Cushing Society. Springfield, Charles C Thomas, 1939, vi, 146 pp.

[6] Except, possibly, Russia.

THREE TRACTS ON FOOD IN BASEL MANUSCRIPTS

LYNN THORNDIKE
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During the last days of August, 1939, which elapsed between the announcement of the Russian-German pact and the German invasion of Poland, I examined in the University and Public library of Basel three tracts on food which—with one possible exception [1]—seemed different from any of those listed in *A Catalogue of Incipits of Mediaeval Scientific Writings in Latin*.[2] My reading of these three tracts in as many different manuscripts began on Friday, August 25th, and was interrupted by the closing of the library Saturday afternoon on account of the local celebration of the Swiss victory at St. Jakob in 1444 over invaders led by the future Louis XI of France, then Dauphin. The terrible European crisis and the possibility that the neutrality of Switzerland might soon be threatened lent poignancy to the occasion, and I watched with keen interest at the bridge over the Rhine the long procession of troops—some in uniforms of the time of the French Revolution—of gilds headed by members in medieval garb, of university students in duelling array, of women in native costume, of choral and musical organizations, of bands of children with concertinas, while so many persons were beating drums that it was difficult to figure where these could all have been stored away. It was good to see and hear the triumph of liberty thus vigorously celebrated by a German-speaking people.

On the subject of foods there were available in the closing medieval centuries Galen's *De alimentorum facultatibus* in the thirteenth century Latin translation by William of Moerbeke and his briefer *De subtiliante dieta* in the early fourteenth century Latin translation by Nicholas of Reggio. Nicholas had also translated the pseudo-Galenic *De nutrimento*.[3] Another pseudo-Galenic work, *De virtutibus ciba-*

[1] Our tract opening, "Cibi vero qui generant . . ." is perhaps identical with that in Vienna Latin MS 2426, 14th century, fols. 141r-142r, which I have not seen and for which the printed catalogue gives only two words of incipit, "Cibi gignentes. . . ."

[2] Thorndike, Lynn, and Kibre, Pearl, 1937.

[3] For incipits and manuscripts or editions of these translations consult the index of Thorndike and Kibre, *A Catalogue of Incipits*. In the sixteenth century trans-

riorum, or, *De dissolutione continua*, had been translated into Latin at Bologna in 1200 by Accursius of Pistoia.[4] From antiquity also came down the cook-book of Apicus,[5] and Anthimus, *De observatione ciborum*,[6] while a *Book of Diverse Diets* of Alexander and others is perhaps partially extracted from Alexander of Tralles. From the Arabic the leading work confined to our particular theme was the *Universal Diets* and *Particular Diets* of Isaac, upon which Petrus Hispanus commented at length in the thirteenth century.[7] But the subject had also been treated by Avicenna, Averroes and Rasis in their general medical works, which are much cited by the anonymous tracts with which we are to be here concerned. The treatise of Albucasis on foods for the sick had been translated from Arabic into Latin by Berengarius Eymericus of Valencia.[8] Although the *Diets* of Isaac have to do with food, it should be remembered that in medieval medicine the word, diet, was often used to include other aspects of daily life such as sleep and exercise, so that a work entitled *Dietarium* may not be concerned primarily or exclusively with food.[9]

A treatise on the preparation of food and drink for the sick [10] is

lations by Martinus Gregorius, printed at Lyons in 1555, *De alimentorum facultatibus* has the different incipit, "De facultatibus quae alimentis insunt . . . ," while *De subtiliante dieta* has also a new title, "De attenuante victus ratione," and opens, "Cum victus ratio attenuans plerisque diuturnis morbis. . . ."

[4] See the discussion by Sudhoff in *Mitteilungen zur Geschichte der Medizin*, 16 (1917), 24-27.

[5] The Academy of Medicine, New York, has a fine ninth century manuscript of this work. Incunabula of it are *Gesamtkatalog* 2267-2268.

[6] A recent edition is *Corpus Medicorum Latinorum*, VIII. i (1928), 1-33.

[7] I have discussed Peter's commentaries in *A History of Magic and Experimental Science*, II, 502-10. Latin MS Naples, Oratoriana XVI, vii, catalogued by Mandarini as an anonymous herbal, turns out upon examination to be Isaac on *Particular Diets*.

[8] Vienna Latin MS 5434, 15th century, fols. 283-321: Dictio de cibariis infirmorum, opening, "Inquit Albucasis in cibariis ex quibus. . . ."

[9] The *Dietarium* of Stephanus Arnaldi, contained in a Latin MS at the British Museum, Sloane 1100, 15th century, fols. 122-163v, includes a brief treatment of gems. The treatise is said to have been published by Arnaldi just before his sudden death: "Explicit Dietarium editum a venerabili magistro Stephano Arnaldi tempore sui obitus repentini."

[10] "De cibis et potibus preparandis infirmis. . . ." See Thorndike and Kibre, *A Catalogue of Incipits*, 1937, col. 171, for various MSS, but its VAp 2392 should be VA 2392, the MS being a Vatican Latin and not Vatican Palatine Latin. The tract also occurs anonymously in Basel D. I. 11, fols. 100r, col. 1-101v, col. 2, "De

variously ascribed to Nicholas of Salerno,[11] Petrus Musandinus,[12] Arnald of Villanova, and John of Gaddesden. I have called attention in publications elsewhere to somewhat similar compositions: a tract composed in the year 1338 by Barnabas de Reatinis, entitled, *De naturis et qualitatibus alimentorum*,[13] and the *De saporibus* of Maino de Maineri, who died about 1364.[14]

Before proceeding to description of our present three treatises on foods, we may give some account of the three manuscripts at Basel in which they are to be found.

Our first treatise on foods is the last item in a long medical miscellany of over three hundred leaves in a fifteenth century hand, Basel D. II. 13. On fol. 1r is a table of contents which, however, omits many minor items. The following analysis of the manuscript's contents is much fuller, with indication of pages covered and incipits. An old pagination, which I indicate in parentheses, has been altered, partly probably as the result of rebinding, so that what were once fols. 3-27 are now 32-56, while 29-36 have become 2-9.

fols. 2r, col. 1—9r, col. 1 (29-36): Vocabularium de herbis et aliis medicinalibus, opening, "Arthemisia bugglosa similis . . ."

ff. 10r, col. 1—16r, col. 2 (37-43): Vocabularium de morborum speciebus diversis, opening, "Allopitia est casus capillorum . . ."

ff. 16v, col. 1—22r, col. 2 (43-49): "Incipiunt excerpta lilii . . ./

cibis et potibus preparandis infirmis videamus et qualiter eorum malitia reprimatur . . ." and in *L'Art Ancien*, catalogue XIX, MS 17, 15th century, fols. 131v-140.

[11] Pansier, *Archiv für Geschichte der Medizin*, II (1909), 32, identified Musandinus as a Salernitan who flourished about 1160 A. D.

[12] A commentary or glosses on the Prognostics of Hippocrates is also sometimes ascribed to Petrus Musandinus. See Thorndike and Kibre, *A Catalogue of Incipits*, col. 594, for several MSS.

Probably to be identified with Musandinus is a Petrus de Mussandal, to whom a *Summa de opiatis* and a *De clysteribus* are attributed by the catalogue of Harleian Manuscripts in the British Museum: Harl. 3719, fols. 14v, 18. So, too, very likely is a Petrus de Musaria, to whom 77 chapters on diseases from fevers to gout are ascribed in yet another MS of the British Museum: Sloane 1124, 13th-14th century, fols. 147-171: "Cum stomachus pro necessitate sui et humani corporis nutrimento tam cibi quam potus sit receptaculum . . ./. . . Explicit tractatus magistri Petri de Musaria."

[13] "Another Treatise by Barnabas de Riatinis of Reggio," *Isis*, 8 (1926), 285-86.

[14] "A Medieval Sauce-Book," *Speculum*, IX (1934), 186-89.

. . . Finis Lilii." But additional recipes follow, and are also interlarded elsewhere through the manuscript.

f. 23v, col. 1 (50): "Incipiunt excerpta gwidonis."

ff. 25v, col. 1—28r, col. 2 (52-55): Vocabularium de gradibus medicinarum, opening, "Quia Galienus medicorum eximius . . ."

f. 29r, col. 1—v, col. 1 (60): Iudicia leprosorum. "Circa iudicia leprosorum 16 sunt notanda . . ./. . . Explicit Iudicium leprosorum gwidonis."

f. 30r, col. 2 (1): Tractatus de pulsibus, opening, "Sumuntur signa de pulsibus. In principio applicatis digitis ad pulsum . . ."

f. 31v, col. 1 (2): ". . . Et hec de pulsibus sufficiant. Hec extracta sunt de libro Richardi de signis."

ff. 32r, col. 1—56r, col. 2 (3-27): Thesaurus pauperum, opening, "In nomine sancte et individue trinitatis qui omnia creavit que deus prescivit . . ."

f. 59r, col. 1—v, col. 2 (58): De quatuor qualitatibus, opening, "Cum in humanis corporibus sunt . . ."

f. 60r, col. 1-2 (59): De urinis, opening, "Si urina habet circulum spissum . . ." Many recipes follow, while ff. 67-70 (66-67, 70-71, old leaves 68-69 having been cut out) are left blank.

ff. 71r-103r (73-105): Macer de virtutibus herbarum metrice, opening, "Herbarum quasdam dicturus carmine vires . . ."

ff. 108v, col. 1—112v, col. 2 (110-114): De venenis magister Petrus de Ebano, opening, "Intelligat vestre sanctitatis reverentia quod septem sunt . . ."

ff. 115-238r, col. 2: listed in the table of contents as Thesaurus pauperum secundum ordinem alphabeti, is by Vitalis de Furno, whose preface fills the first page, opening, "In isto libello continentur remedia contra diversas infirmitates ad que possunt pauperes recurrere et subito invenire . . ." At

ff. 115v-118v is an alphabetical index in three columns grouped under 116 numbers corresponding to that many leaves following.

f. 119r, rubric, "Incipit libellus editus per dominum Vitalem de

Furno quondam Cardinalem de diversis remediis contra diversas infirmitates"; incipit, "Sunt autem ista remedia posita et ordinata secundum ordinem alphabeti . . ."

f. 235r, rubric, "Explicit libellus utilis pro pauperibus et patientibus qui non possunt habere copiam medicorum."

ff. 235v, col. 1—238r, col. 2 are occupied by another index.

ff. 239r-250v: De aquis medicinalibus, opening, after a table of 64 caps., at f. 239v, "Testante philosopho scientia multiplex fit per transitum plurimorum . . ."

ff. 251r, col. 1—262v, col. 2: Compendium de speculabilibus in medicina, opening, "Avicenna. Medicina est scientia qua humani corporis dispositiones . . ."

f. 263r: "Nota febris sic describitur secundum Avicennam . . ."

f. 264r-v: charts and graphs.

ff. 265r-266v: what the table of contents describes as, "Contra senectutem medicina in epistola cuiusdam doctoris," appears to be the letter of Roger Bacon to pope Innocent IV on avoiding the ills of old age. Incipit, "Scrutatus sum meo tempore omnia armaria Latinorum . . ."

ff. 267r-268v: Utrum in regimine sanitatis competant diversi cibi in eadem refectione, opening, "Ad maiorem evidentiam eorum que dicuntur . . ."

ff. 269r, col. 1-272r, col. 1; Canones in medicinis servandi diligenter (from Avicenna).

ff. 275r-282r:
opening, "Dicemus quod res medicationes ex una trium . . ."

f. 283r: Regule conservande sanitatis. De aere. Purus aer prodest menti et corpori . . ."

f. 287v: Sequitur de cura emoroidinis.

ff. 287v-289r: Sequuntur excerpta de alio libello scripto ad papam de regimine sanitatis, opening, "Sanguis est pater vite . . ."

ff. 289r-290r: Excerpta de pomo vite.

f. 290r-v: Excerpta de secretis secretorum.

ff. 293r-309r: anon. de cibis, opening, " Magister reverende spero vos omnino ab egritudine . . . " and closing, " . . . qui omnium princeps et creator verus existit qui sit benedictus in secula seculorum. Amen."

Basel D. III. 2, in which our second treatise on foods is found, is a paper folio of the fifteenth century with its leaves unnumbered. Except for an abbreviated version of the *De proprietatibus rerum* of Bartholomaeus Anglicus, here ascribed to Albertus Magnus, the component tracts of the manuscript are medical. They include, in addition to the tract on foods, the pest tract of Johannes de Tornamira; a medical treatise which opens, " Dimittis causas et essentiam egritudinis solum veniam ad actum curativum qui perficitur per debitam sex rerum . . . "; a *Compendium de regimine sanitatis*, opening, " Sicut scribitur ab Avicenna primi canonis capitulo de causis sanitatis . . . "; a *Consilium* on skin diseases; another *Regimen sanitatis*, opening, " Primo interrogastis ut vobis libellum conservationis sanitatis scriberem . . . "; the treatise by Arnald of Villanova on conserving youth and retarding age, addressed to the king of Jerusalem and Sicily; the *Circa Instans* of Platearius; lists of the names of herbs, and *Quid pro quo*, which here opens, " Quoniam que sunt utilia in curationibus egritudinum inveniri ubique non possunt . . . " The table of contents on the flyleaf of this manuscript is inaccurate.

The third treatise on foods occurs in Basel D. III. 19, a manuscript of octavo size in different hands of the fourteenth and fifteenth centuries with unnumbered leaves. The first 56 leaves according to my count are occupied by the lectures of Gerard de Solo on Rasis, *Nonum Almansoris*, written in a neat and legible hand. There follows (at ff. 57r-95r) a work on medicinal simples opening, " Cassia fistulis est de medicinis in quibus fiducia est . . . " After a blank leaf and a half come, in a hand which is more humanistic but still legible, Hippocrates' *Aphorisms*, *Prognostics* and *Regimen acutorum*. Then ensues the pest tract of John of Saxony in a very negligent fifteenth century hand which occupies more of the page. After two blank leaves the handwriting again changes, becoming neater but very abbreviated and difficult to read. The treatise written in this script is that of Arnald of Villanova on wines and fills eight

leaves. In a hand which combines the difficulties of the two preceding is a tract of three pages on the elixir and one of four pages, *De aqua balsami mirabili*, opening, " Recipe terbentine lb. i mellis. . . ." After four blank leaves comes a *Tractatus de reumate*, of which there is a more legible version, with an introduction and conclusion which are here missing, in Basel D. III. 23, fols. 1r-23v.[15] Finally, after an intervening page of recipes, comes, in a hand different from any other of the manuscript, the treatise on foods, followed by a brief *Anathomia* or *De positione corporis interioris*, filling three leaves and opening, " Quoniam humani corporis interiorum membrorum positiones. . . ."

Our first treatise has a practical purpose and character in that it is addressed to a " reverend master " who has been ill and is liable to a relapse unless, as he is warned, he observes those rules of health which permit medical science to preserve those who are well in their sanity and to recall the sick to health. To this end he should know the natures of varied foods and drinks in order that he may select those which are good for him and reject those which disagree with him. In closing the anonymous author describes his treatise as a complement to and derivative from the better utterances of Galen, Avicenna, Rasis, Hippocrates, " and other Greek medical writers." His book may not be of great utility, yet an assiduous peruser of it can acquire some knowledge and save himself from threatening ills. But what good there is in the work is not from the author but from the Prince and Creator of all.

The order of presentation followed is informal and somewhat popular. We begin with bread because it is brought first to the table and continue with beverages because they are the second thing to be placed on the board. Wheaten bread is most nutritious and is improved by using plenty of yeast and a moderate amount of salt, as Rasis says. Bread cooked in a pan (*qui in patella coquitur*) with eggs and fat, and all bread that is not well done, is difficult to digest, produces pain in the belly, and cannot be eaten frequently except by those who engage in hard labor or violent exercise. Unleavened

[15] Basel D. III. 23, fol. 1r, " Tractatum de reumate quatuor capitulis distinguimus, in primo capitulo dicentes . . ."; f. 1v, rubric, " Capitulum primum de diffinitione reumatis et quomodo generetur et que et quot sunt cause reumatis "; fol. 2r, incipit, " Volens igitur reume diffinire oportet modicum philosophari. . . ."

bread is hard to digest, descends from the stomach slowly, causes pain in the belly, opilation in the liver, and stone in the kidneys. Wherefore its use is to be avoided, for which Rasis and Avicenna are cited as authorities. The author proceeds to treat of bread made of white wheat (*siligineus*) and barley bread (*ordeatus*), and then of different drinks—beer, a mixture of honey and water, wine.

Turning to flesh, our treatise discusses that of such animals as the kid, ram, goat, calf, sheep, pig, rabbit, cow, deer, dove, duck, and of flying birds in lakes and rivers. The meat of chickens, pheasants, partridges, Berkhun, Haselhun, nightingales, blackbirds, larks, all other mountain birds, and that of small fish without viscosity makes the blood thinner. The juice or broth of decrepit hens is beneficial for choleric persons and for melancholy diseases, but also comforts the virtues of youth. But the juice of chickens tempers the humors more, while that of hens augments the intellect and clarifies the voice. The flesh and brains of hens are good for cerebral hemorrhage. The juice of cocks helps palpitation of the heart and pains in the joints as well as asthma. The juice of an aged cock taken with fern and anise relieves colic and stomach-ache from wind. Taken with safflower and powdered senna, it dissolves melancholy. For which are cited William (Saliceto) of Piacenza in his *Synonyms* and Avicenna in the *Canon*.

Rasis and Avicenna are cited that meat roasted over the coals is very nutritious but fried in fat produces a slight indigestion or nausea (*fastidium*). Meat cooked in pans in the oven is bad. That roasted on a spit is heavy and nutritious but indigestible. Our author further considers meat boiled with water and then chilled, or cooked with wheat and milk. Meat cooked with eggs and pepper is for wintertime. Rasis, Avicenna and Isaac are cited. Next are taken up such parts of animals as the head, brains, marrow and liver. Liver is termed hot, slowly digested, and heavy.

Since meat is not good without salt, our author turns next to the nature of the latter and quotes book eighteen (apparently a mistake for nineteen) of *De proprietatibus rerum*.[16] Salt jumps from the fire, excites the appetite, and cattle who eat it abound in milk and cheese. Its effects on the human body are further detailed.

[16] The work of Bartholomaeus Anglicus in the thirteenth century.

The author then announces, " Now I will speak of the order of taking foods." On rising in the morning, one should rub one's head hard, and wash face, hands and eyes with cold water in summer and hot water in winter. Next brush the teeth with finger and water, then make one's peace with God in prayer, and then exercise oneself in pure places. Let food be prepared so soon as one begins to crave it but not until. If there are diverse foods at the same meal, it is better to eat what is light before what is heavy, and what is softening before what is stiptic. Our author advises to begin a repast with chicken broth, fresh beef broth, pea soup, or soup mixed with cheese. Next should be eaten meat easily digested like chicken, partridge or pheasant. Last should come beef, cheese and pears which possess stipticity, because heavy things eaten last press the food down to the bottom of the stomach where the digestion is good because the bottom of the stomach is hotter and more potent in digesting. The upper part of the stomach is nervous, cold, weak in digesting. Hence, when food floats in the stomach, it is not digested. The reverend master is also warned not to take much wine at the beginning of a meal, and that wine mixed with food hinders digestion. After a meal he should rest standing or walk a little at a time, because any violent motion corrupts the food. After a little one may take a nap according to one's temperament.

Galen and Bernard Gordon are now cited, but the author of our treatise soon " recurs to experience which rectifies all, since 'tis the lord and master of all things."

Because cabbage, peas and gourds are sometimes eaten with meat, our author speaks next of them. Red cabbage is bad enough, but white cabbages are worst of all. Peas served with their pods are flatulent but without the pods are nourishing. Other vegetables which are considered are mallows, *orach*, spinach, fennel, *baucia*, rape, onions, garlic and lettuce. Melons turn quickly into red choler, corrupt, and provoke urine. Anyone who eats melons will suffer from choleric passion. Cucumbers, on the other hand, are represented as cooling, as relieving thirst, chilling the stomach and provoking urine. Treatment of other fruit is postponed to the section upon foods which are permitted on Fridays and other fast days. Because cheese is eaten after meat, our author discusses it next, quoting the *Etymologies* of Isidore of Seville.

Then inasmuch as meat, and fish too, is cooked with spices, he speaks of seeds and spices. White pepper is not available in his region, so that *cathapucia transmarina* is substituted for it. Cummin seed, cinnamon, cloves, saffron, galingale, zedoary are other seasonings discussed with frequent citation of the *Circa Instans* of Platearius. These are "what we commonly use in foods, so I have made mention of them only." Our author says that zedoary resists aconite and is a tyriac for all poisons.

Coming to foods for Fridays and Lent, the author speaks first of fresh fish and then of salted fish. All fish are difficult to digest and remain long in the stomach. Salted fish are by no means to be eaten except on a day on which one intends to take physic (*nisi in die quo quis medicari se voluerit*). Since nuts are eaten after fish as cheese is after meat, they are discussed next, after which the author speaks of fruits which are eaten on these fasting days. His list comprises figs, grapes, almonds, pears, apples, peaches, plums, cherries, citrons and pomegranates. He concludes with foods appropriate for Saturdays (*sabbatis diebus*): eggs, milk, cheese, *caseatum*, butter, whey and fish.

The second treatise on foods with which we are here concerned, after imploring divine aid in the words of Rasis,[17] begins like the first treatise with the subject of bread.[18] The author asserts that all masters of medicine are agreed that the best food for men in the fourth or fifth clime is wheat somewhat tempered and prepared. The *Colliget* of Averroes and Rasis are cited, while the chapter on bread in the fifth book of the *Canon* of Avicenna is quoted to the effect that bread hot from the oven is harmful, provoking thirst and swimming in the stomach because of its vaporous humidity. After expressing a similar opinion to that of our first treatise concerning unleavened bread, mentioning bread made of *simula*[19] and barley bread, and stating that bread of white wheat (*siligo*) is next best to wheaten

[17] Basel D. III. 2, rubric: "Incipit libellus de virtutibus cibariorum." Then follows: "In primis ad inchoandum et perficiendum opus divinum auxilium imploro iuxta illud Rasis. . . ."

[18] *Ibid.*, rubric, "De pane"; opening, "Concordati sunt omnes magistri medici quod melior ex cibis. . . ."

[19] I am inclined to think that this means semolina rather than simila, the finest wheat flour.

bread, our author explains that he does not mention other kinds of bread, "because they are not in use among us."

Turning next to meat, our author asserts that it nourishes the body more than other foods. After discussing meat in general, the flesh of flying animals is first considered and then that of non-flying animals. In the latter category the flesh of kids is pronounced the best. Averroes and Rasis rank it next to chicken.

Subsequent rubrics deal with the members of animals, with fish, milk, cheese, eggs, fruit, grapes, apples, pears, citrons, peaches, plums, berries, nuts, almonds and pomegranates—a list very similar to that of the first treatise. Somewhat the same vegetables, too, are considered: lettuce, leeks, spinach, rape, garlic, and peas. Barley, honey, salt, pepper, ginger, saffron, cinnamon, mace, cloves, nux muscata, galingale and cold vinegar (*acetum frigidum*) complete the contents. After he has apparently finished,[20] however, our author adds a supplement to the effect that wine taken with meals should be clear and with a good bouquet and taste, as is stated in the compendium *De regimine sanitatis*. It also is advisable to drink only one kind of wine at a meal. If two wines are drunk, the better and stronger should come first.[21]

Our third tract on foods opens, after an introductory paragraph, with a brief enumeration of those which generate good humors.[22] There follows a like presentation of those which generate bad humors, of foods which are very nutritious and of those which nourish little, of those which are easy to digest and those which are difficult to digest, of those which are fattening and those which make one thin. A blank page then intervenes, but after it what would seem to be the same treatise is continued in the same handwriting, which differs from any preceding hand in the manuscript.[23] Soon are discussed grain, bread, barley, millet, beans, chickpeas, lentils, and other foods. After the accounts of butter, milk, wine and waters,

[20] "Et hic sit finis de virtutibus cibariorum. Tamen volo unum adiungere. . . ."

[21] ". . . melius vel fortius proponatur. Et sic est finis de virtutibus cibariorum usitatorum."

[22] Basel D. III. 19, rubric, "Sequuntur de cibis generantibus bonos humores"; incipit, "Cibi vero qui generant bonos humores. . . ."

[23] The text resumes: "De cremento humorum. Sciendum est quia quotiens aliquis quatuor humorum. . . ."

there occurs the caption, "Of the natures of the four elements."[24] Matters of health are treated for a few pages, and then we read, "Expliciunt diete particulares." But the treatise seems shorter than and not identical with the *Particular Diets* of Isaac.

By way of supplement I may perhaps add a reference to a treatise on foods outside of Basel manuscripts. Robertus Constantinus, in his sixteenth century bibliography of noteworthy writings printed or manuscript in libraries of France and England, lists a *Liber de alimentorum facultatibus* by Symeon, a master of Antioch, as extant in a Greek manuscript.[25] I have been unable to find any other notice of this Symeon, whose book was perhaps merely a copy of or commentary on that by Galen of the same title.

Attention may also be called here to one more discussion of food, found not in a Basel but a Zurich manuscript.[26] Sudhoff in one of his series of articles on Pestschriften briefly noticed the author, Conrad Monopp, and two other manuscripts of the work at Berlin and Leipzig, but neither our Zurich manuscript nor the contents of the work.[27] While the treatise is a general medical compendium on

[24] "De naturis IIII^{or} elementorum. Corpus hominis ex quatuor constat humoribus. . . ."

[25] Robertus Constantinus, *Nomenclator insignium scriptorum quorum libri extant vel manuscripti vel impressi ex bibliothecis Galliae et Angliae: Indexque totius bibliothecae atque Pandectarum . . . C. Gesneri*, Paris, 1555, p. 162.

[26] Zurich Car C 111, 1445-1449 A. D., fols. 1r, col. 1-61r, col. 1: rubric, "Incipit compendium de regimine sanitatis exceptum per magistrum Conradum Monopp de Ruedlingen pro tempore doctorem scolarem ecclesie Augusten. ex omnibus dictis doctorum medicine artis, cuius prologus tria habet capitula, Primum de utilitate necessitate prioritate et universaliter de fine huius scientie." Incipit, "Universorum conditor deus in donis suis. . . ." At fol. 2v, col. 2, the first chapter of the first Doctrina of the first Tractatulus of the first book opens: "Infans cum primo nascitur. . . ."

The subject of food is considered in the second book at fols. 20v, col. 2-51r.

[27] Karl Sudhoff, "Pestschriften aus den ersten 150 Jahren nach der Epidemie des 'Schwarzen Todes,' 1348," *Archiv. für Geschichte der Medizin*, XIV (1922-1923), 136. The other MSS are Berlin lat. 4to 373, 15th century, fols. 1-132, and Leipzig Folio 1181, fols. 1-71. Although Sudhoff noted that an anonymous pest tract followed in the Berlin MS at fols. 133r-149, he does not seem to have remarked that Conrad's Compendium de regimine sanitatis devoted the third Doctrina of the second tractate of the first book to regimen in the time of epidemics. This section is in seven chapters and occupies fols. 15r, col. 1-18v, col. 2 in the Zurich manuscript, where it opens: "Quia nonnumquam unius climatis aut plurium. . . ."

Sudhoff gave the place name of the author's home as Rüd in one MS and

the preservation of health, it devotes half its space to bodily nutriment. First are given general rules for eating and drinking. Then particular foods and drinks are considered in this order: cereals and bread, vegetables, herbs and their leaves and roots, fruit, nuts, flesh of quadrupeds, of birds, the cooking and seasoning of meat, the preparation of fish and crabs, food from living animals like eggs and milk and cheese, water, wine, mixed or manufactured drinks, with a concluding section on tastes and qualities. To indicate the detailed character of the treatment I reproduce here the chapter headings for the second and third tractates of this second book on nutriment.

fol.	col.	
24v	2	Tract. 2 de bonitate et malicia ciborum
		Doctrina 1 de omni cibo qui fit ex granis
		cap. 1 de generali distinctione ciborum et de pane
25v	1	" 2 de pulmentariis ex granis frumenti
26r	1	" 3 de leguminibus
26v	1	Doctrina 2 de nutrimento terre nascentium et vegetabilium aliorum a granis et leguminibus
		cap. 1 de cibis factis ex foliis herbarum
28r	1	" 2 de quibusdam herbis in speciali magis consuetis ad commedendum secundum ordinem alphabeti
28r	2	" 3 de radicibus quarundam herbarum esibilibus
29r	1	" 4 de quibusdam fructibus crescentibus super terram consuetis ad commedendum
29v	1	Doctrina 3 de fructibus arborum
		cap. 1 de regulis habendis ad usum arbutorum
30v	1	" 2 de piris pomis coctanis nespulis granatis dactilis ficubus et uvis
31v	2	" 3 de fructibus lapidosis
32r	1	" 4 de nucibus
32v	2	Doctrina 4 de omni nutrimento carnium quadripedum
		cap. 1 de generalibus conditionibus carnis secundum diversitatem locorum in quibus vivunt et nutriuntur animalia
33r	2	" 2 de variis conditionibus carnis quadrupedum secundum varietatem complexionum etatum nutrimentorum sexium et temporum

Budlingen in the other, interpreting the latter as intended for Büdingen. He said further, "Ob unter Augustensis wirklich Augsburg gemeint sei, kann bezweifelt werden."

fol.	col.	
34r	1	" 3 de electione carnis secundum etatem singulorum animalium gressibilium tam domesticorum quam silvestrium
34v	1	" 4 de qualitatibus carnis commestibilibus digestibilium domesticorum et silvestrium in speciali
35r	1–2	" 5 de comparatione partium animalis commestibilis penes ipsarum electionem et bonitatem vel maliciam in generali
35v	2	cap. 6 de singulis membris in speciali
36v	1	Doctrina 5 de carne volatilium
		cap. 1 de electione carnis volatilium secundum complexiones etates loca in generali
37r	1	" 2 de carne volatilium in speciali
37v	2	" 3 de generibus conditionibus ac complexionibus piscium et cancrorum
38r	1	" 4 de electione piscium et eorum varietate multiformi
39r	2	Doctrina 6 de preparatione carnium
		cap. 1 de coctione et assatura et frixatura
39v	2	" 2 de condimento salis et unctuositatis
40v	1	" 3 de condimento mellis zukkari et acceti
41r	1	" 4 de condimento specierum aromaticorum
41v	2	" 5 de aliis . . . que non computantur inter species aromatices ut crocus ciminum carni granum iuniperi sinapium agresta salsa
42v	1	" 6 de preparatione piscium et cancrorum
	2	Doctrina 7 de nutrimento quod sumitur ex animalibus vivis ut sunt ova et lacticinia
		cap. 1 de ovis
43r	1	" 2 de diversitate ovorum ex artificio preparandi
43v	1	" 3 de lacte in generali et eius bonitate
44r	1	" 4 de nocumento lactis
	2	" 5 de diversitate lactis secundum substantiam et secundum sui preparationem et secundum animalia a quibus provenit
44v	2	" 6 de caseo de brodio casei de placentis archotis et pastillis
45r	1	Tract. 3 de qualitatibus potuum
		Doctrina 1 de aquis
		cap. 1 de aquis fluentibus et de electione aquarum bonarum
45v	1	" 2 de aquis stantibus et malis
46r	1	Doctrina 2 de vino
		cap. 1 de vino quo ad eius bonitatem et nocumentum

fol.	col.	
46v	1	cap. 2 de diversitate vini et eius electione secundum se et in comparatione ad homines
47r	1	Doctrina 3 de potibus confectis
		cap. 1 de vino artificialiter confecto
47v	2	" 2 de aliis potibus arte confectis

Finally some sixteenth century questions and opinions on food may be adduced for brief comparison with our late medieval manuscripts. They are from the 1578 edition of the work in French by Laurent Joubert on popular errors in matters of medicine and health and are mere topics proposed for discussion but which the author never completed. One is that the flesh of the pig is the most nourishing of all; another, that rats, cats and other animals are as good to eat as those to which we are accustomed. Another thesis is that to eat truffles or mushrooms is a sign of a disordered appetite, while it is questioned whether it is true that truffles, artichokes and oysters make a man more wanton sexually. The opinion is advanced that good fish is better in summer than meat even for choleric and feverish persons. It is asked why we indulge in entrées and desserts, which are prejudicial to health, and in a variety of viands at a single meal, when physicians forbid this. Also whether lettuce and whether fruit is better eaten at the beginning or end of a meal. An entire book was to have been devoted to fruits and salads.[28] Two of its chapter heads aver that salad ought to be much stronger of salt than vinegar, and that lettuce is more wholesome with honey than otherwise. The former chapter was also to discuss why it was said that four persons were needed to make a salad properly.

[28] Laurent, Joubert, *Erreurs populaires au fait de la médecine et régime de santé*, Bordeaux, 1578, table of contents for books xi, xii and xiii.

NOTES ON THE PLAGUE TRACTS IN THE BOSTON MEDICAL LIBRARY

HENRY R. VIETS
AND
JAMES F. BALLARD
Boston Medical Library

In view of the widespread Black Death of the fourteenth and fifteenth centuries and the economic devastation following in its wake, it is not surprising that plague was regarded as a special manifestation of divine displeasure. Hence, sermons, proclamations and tracts were issued in great numbers in the Middle Ages in an attempt to check the scourge spreading across Europe from the Far East. Not a few of these writings have been preserved in manuscript form, but those that were printed in the fifteenth century are better known. Dr. Klebs has long been a profound student of this literature and his two publications, the results of collaboration with colleagues, are the standard references for pest writings in their printed form. The first volume, with E. Droz, *Remèdes contre la peste. Fac-similés, notes et liste bibliographique des incunables sur la peste,* issued in Paris, 1925,* lists 130 editions of plague tracts and reproduces in facsimile three French tracts and parts of two others. The Klebs-Droz list was augmented by a new listing in 1926, when Klebs, in collaboration with Karl Sudhoff, published *Die ersten gedruckten Pestschriften,* in München. Without changing, except slightly, the numberings of the pest incunabula, as given in the Klebs-Droz list, the new rendering gave more details about each book and made additions to the list. The most important new material is *Ordnung der Gesundheit* or *Regimen sanitatis zu Deutsch,* for in the first of this group (1472), under *Lere von der Pestilentz,* is found the earliest known printed tract against the plague.

With the aid of these two lists, the plague tracts in the Boston Medical Library have been reviewed. The list follows, preceded by the Klebs-Sudhoff identification number. The books are listed in the approximate form and followed by the numbers used by Klebs in his *Incunabula scientifica et medica,* Bruges, 1938.

* Also issued the same year in Paris with the title in English.

Plague Tracts in the Boston Medical Library

Klebs-Sudhoff.		*Klebs.*
No. 6.	Benedetti, Alessandro. *De observatione in pestilentia.* Venice, Gregoriis, 29 July 1493.	172.1
No. 8.	Biel, Gabriel. *Sermo contra pestilentiam medicinalis. Fuga pestis.* In: *Sermones de tempore.* Tübingen, Frid. Meynberger, 10 March 1500.	
No. 9.	Brunschwig, Hieronymus. *Pestilenz.* [Strasburg], Grüninger, 19 Aug. 1500.	228.1
No. 20.	Canutus. *Regimen pestilentiae.* [Nürnberg, anon. pr., 1482].	245.2
No. 38.	Capelluti, Rolando. *De curatione pestiferorum apostematum.* [Rome, anon. pr., 1485].	248.3
No. 40.	Cermisone, Antonio. *Ricette contro pestilenza.* [Milan, anon. pr., 1483].	264.2
No. 44.	Ficino, Marsilio. *Consiglio contra pestilenza.* Florence, Ripoli, 1481.	396.1
No. 51.	Gentile da Foligno. *Consilium contra pestilentiam.* [Colle, anon. pr., 1478].	445.2
No. 53.	Guainerio, Antonio. *Opera medica. De peste et venenis* only. [Venice, anon. pr., 1487].	480.4
No. 62.	[Jacme, Jean]. *Tractatus de pestilentia.* [Augsburg, anon. pr., 1480].	542.1
No. 65.	Jung, Ambrosius. *Tractatus de pestilentia.* Augsburg, Schönsperger, 20 Nov. 1494.	568.1
No. 70A.	Diedo, Francesco. *Vita S. Rochi.* [Mainz, anon pr., 1494].	335.6
No. 72.	Manfredi, Girolamo. *Tractatus de peste.* Bologna, [anon. pr., 1482?].	656.1
No. 75.	*Lere von der Pestilentz.* In: *Regimen sanitatis zu Deutsch: Ordnung der Gesundheit.* Part of: *Lehre und Unterweisung.* Augsburg, Bämler, 22 Apr. 1472.	596.1
No. 77.	Pintor, Pedro. *Aggregator sententiarum de pestilentia.* Rome, Silber, 20 Feb. 1499.	779.1
No. 87.	Razi. *De pestilentia.* In: Valla, Giorgio. *Collectio.* Venice, Bevilaqua, 30 Sept. 1498.	1012.1
No. 98.	Soldi, Jacopo. *De peste.* Bologna, Schriber for Th. Bononia, 1478.	921.1
No. 112.	Pietro da Tossignano. *Consilium pro peste evitanda.* In: Ketham, Johannes de. *Fasciculus medicinae.* Venice, Gregoriis, 15 Oct. 1495.	573.2
No. 113.	——— Venice, Gregoriis, 28 March 1500.	573.3

Klebs-Sudhoff.		*Klebs.*
No. 115.	*Tractat von der dotlichen Sucht der Pestilenz.* [Cologne, anon. pr., 1477].	986.1
No. 120.	Valasco de Tarenta. *De epidemia et peste.* [Basel, anon. pr., 1474].	1007.2
No. 125.	Valasco de Tarenta. *De epidemia et peste.* In: Pietro d'Abano. *De venenis.* [Padua], Cerdonis, 18 Dec. 1487.	774.7
No. 126.	Valasco de Tarenta. *De epidemia et peste.* Hagenau, Gran, 23 Nov. 1497.	1007.3
No. 130.	Widman, Johann, dictus Meichinger. *De pestilentia.* [Biel, Gabriel]. *Fuga pestis.* Tübingen, [anon. pr.] for F. M. [Frid. Meynberger, 1501].	1049.1

Additional Plague Tracts and a Broadside in the Boston Medical Library (Not in Klebs-Sudhoff)

	Klebs
Cermisone, Antonio. *Consiglio per preserv. della peste.* [Naples, anon. pr., 1475].	263.1
Diedo, Francesco. *Vita S. Rochi.* [Milan, anon. pr., a. 1, June 1479].	335.1
[Jacme, Jean]. *Remède contre fièvre pestilencieuse.* [Paris, anon. pr., 1485].	543.1
Bustis, Bernardinus de. *De pestilentia signis, causis et remediis.* In: *Rosarium sermonum.* Venice, Arrivabene 1498. G. W. 5: 5807. B. M.C. v: 387.	
"Pestblatt" (*German*). Broadside, 402 x 242 mm. [Augsburg, ca. 1473].	

"Lere Von Der Pestilentz," 1472

The first printing in book form on the pest is recorded by Klebs and Sudhoff (No. 75) as a *Lere von der Pestilentz* (Fig. 1). It forms chapter 3 of the *Regimen sanitatis zu Deutsch: Ordnung der Gesundheit* (Klebs 828.1) printed by Joh. Bämler, at Augsburg in 1472. The *Regimen* consists of leaves 61 to 110 in the series of texts issued by Bämler, the whole volume being usually known as the *Lehre und Unterweisung* (Klebs 596.1), after the first tract. There are only two recorded complete sets of the *Doctrine and Instructions*, one in the Landesbibliothek at Wolfenbüttel and the other as noted above.* A copy in the British Museum [1] lacks leaves 51 to

* The volume in the Boston Medical Library has had an interesting and adventurous career. It was sold in a Czechoslovakian auction to a Continental book-dealer.

110. Sudhoff [2] has made a study of the Bämler volume, using the Wolfenbüttel copy, which has a *Register* leaf. The *Regimen sanitatis zu Deutsch* appears to be the first "medical" book printed in German, the colophon being dated, the Eve of St. Georges Day, 1472 (22 April).[3]

The *Regimen sanitatis*, by an anonymous author, is referred to as

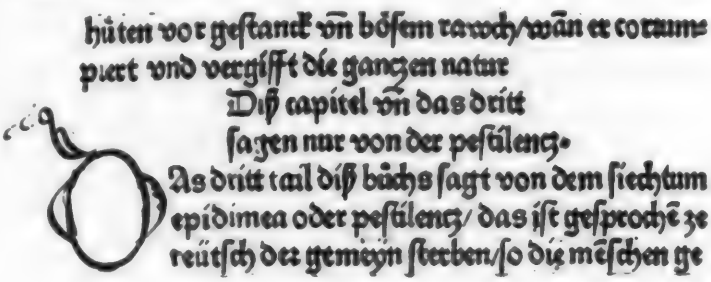
hüten vor geſtanck vñ böſem rawch/wãn er corrum-
pirt vnd vergifft die ganczen natur
Diß capitel vñ das dritt
ſagen nur von der peſtilencz.
As dritt tail diß buchs ſagt von dem ſiechtum
epidimea oder peſtilencz/ das iſt geſprochẽ ze
reütſch der gemeyn ſterben/ſo die meſchen ge

Fig. 1.

Page of the *Regimen sanitatis zu Deutsch* showing beginning of the chapter *von der Pestilentz*, Augsburg, 1472.

zu Deutsch or *Germanice* to distinguish it from the *Regimen sanitatis Salernitanum*, an entirely different book. The texts are commonly confused; Garrison [4] did so in listing the medical incunabula in the Army Medical Library. The *Salernitanum* was not printed until 1480. Both are clearly separated in Klebs' *Short Title List*.[5]

He broke up the volume, used the original binding for the part containing the *Regimen sanitatis zu Deutsch*, catalogued the unbound *Griseldis* of Petrarca separately and put the remnants on his shelf. The *Regimen* was sold by private sale. The *Griseldis* was bought from the catalogue and the remnants were rescued from the book-dealer's shelf by Lathrop Harper of New York, who knew of our interest in the matter. The parts were re-assembled in their original binding in Boston.

[1] Catalogue of books printed in the XVth century now in the British Museum. London: 1912. 2: 331.

[2] Sudhoff, K. Deutsche medizinische Inkunabeln. Leipzig: 1908. Pp. 8-9.

[3] Bond, John J. Handy-book of rules and tables for verifying dates within the Christian Era. London: 1889. p. 155.

[4] Garrison, F. H. Check-list of incunabula in the Army Medical Library, Washington, D. C. *Bull. N. York Acad. M.* 2 S, *6*: 401-435, June, 1930. Item 357, p. 428.

[5] Klebs, Arnold C. Incunabula scientifica et medica. Short title list. Bruges: 1938.

Queste sono recepte facte quasi tute da Magistro Antonio cermisono contra la pestilentia.

LOSSE PRESERUATIUE DA ESSE PESTILENCIE.

Optima cossa e afugir presto da lo logo doue e la pestilentia & lõge. & tornare tarde chil puo fare: pur quando se stesse nel loco de la pestilentia usi le infrascripte cosse.

Uno di meglioni remedij che se possa fare si e usare laceto con tuti li cibi che mangia.

Porti ancho za una spõga i mane cõ due parte d'ace to:e la terza parte d'aqua de rose odorãdola spes so: maximamente quando na fuora di casa.

Del dicto aceto se ne bagni spesse volte li polsi la bo ca:e le narise aspergendo alcuna volta la camera de dicto aceto.

Pillule nõ solutiue ma cõfortatiue. Toglia una drag ma de boliarmino:doe dragme di terra sigillata siruppo de cedro quãto basta p cõficere esse pillu le:& si faciano nove pillule:& se togliano due vol te la septimana tre bore vanti al disnare.

Fig. 2.

The first page of the *Ricette contro pestilenza* of Antonio Cermisone, 1483.

IN NOMO DE DIO ET
DE LA VIRGINE MA
RIA INCOMENSA LO
CONSIGLIO DE MIS
sere antõi sermosone colo quali mol
ti legendo lo se portano preseruare
& sanare dala pestelentia senza me
dico luccedendo el caso.

Remedii efficacacissimi contra la peste cõ
liquali molti sono stati da essa peste libera
ti intra li quali remedii se crede essere sum
mo quello dela poluere infrascripta nel. v
capitolo. auenga chi ancora li altri seno da
fare ma ĩ quisto male de peste e besõgnio
prouedere prestissima mente si chi sia Suc
corso alo infirmo Innanzi che passano qua
tro ore da poy lo aduenimento del male e
ancora piu presto z quanto piu se tarda ra
adprouedere tanto pegio & se per caso nõ
se possesse faf nessune deli altri remedii snõ
se lasse per nullo modo la poluef infrascri
pta la quale poluef se debia pigliaf. innãzi
che passeno xii. ore dal principio de m ale

Primo quãdo lo homo se sente ferito dalo
peste subito si corra ad cauaf el sangue qñ

Fig. 3.

The first page of the *Consiglio* of Antonio Cermisone, [1475].

"Consigilo" of Antonio Cermisone, [1475]

Antonio Cermisone of Padua, professor of medicine at Pavia, who died in 1441, wrote two tracts on the pest in Italian. The better known, *Ricette contro pestilenza*, was first published in Milan in 1480 (Klebs 264.1) and the second edition, also in Milan, in 1483 (Klebs 264.2). The second is listed as No. 40 (Klebs-Sudhoff) in the Boston Medical Library holdings noted above. (Fig. 2.)

The other tract by Cermisone, *Consiglio per preserv. della peste* (Klebs 263.1), has only recently been described by Accurti,[6] using the Samonati copy, now in the Boston Medical Library. It was

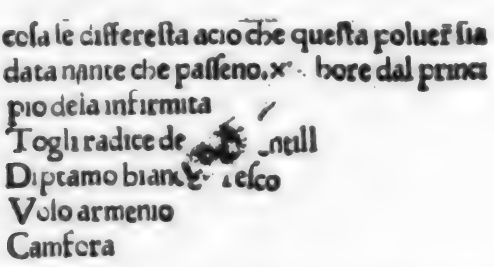
cosa le differesta acio che questa poluer sia
data nante che passeno. x . hore dal princi
pio dela infirmita
Togli radice de [illegible] ntill
Diptamo bianc[illegible] resco
Volo armenio
Camfora

Fig. 4.

"Dropped type" in the *Consiglio* of Antonio Cermisone.

probably printed in Naples by Jodocus Hohenstein, in 1475 (Fig. 3). The *Ricette* appears to be a condensation of the *Consiglio*, issued four or five year later from another city.*

"Vita S. Rochi" of Francesco Diedo, [1497]

Six editions of the *Vita S. Rochi*, four in Latin and two in Italian, are noted by Klebs in the *Short Title List* (Klebs, 335.1 to .6). The first has only come to light since the publication of the Klebs-Sudhoff list (Klebs, 335.1; B. M. C. VI: 760; G. W. 7: 8329). Klebs assigns it to an anonymous printer, Milan, after 1 June 1479. It

[6] Accurti, Thomas. Aliae editiones saeculi XV. . . . Florence: 1936. Item 9, p. 6.

* Incidentally, the Boston Medical Library copy of the *Consiglio* shows a good example of a completely "dropped type" (Fig. 4).

contains a four-page introduction by Diedo, dated *Brixie Kall' Iuniis MCCCCLXXVIIII,* the *Rochi vita* by Diedo and a short letter by Petrus Ludovicus Maldura. Of the first edition, only one other copy is recorded, that in the British Museum.[7]

The sixth edition, 1494 (Klebs, 335.6; Klebs-Sudhoff, 70A; B. M. C. 1: 49; G. W. 7: 8332) is also in the Boston Medical Library and is noted in the list above. The title-page, *Petrus Ludovicus Maldura In Uitam sancti Rochi Contra Pestem Epidimie . . .*, carries a fine woodcut of S. Roch, with his staff and dog, and an angel applying a remedy from a pot to his injured left inner thigh. In the book is reprinted Diedo's *Rochi vita,* without acknowledgement, the *Officium sancti Rochi* and a poem, *Carmen elegiacum,* by *Theodoricus Gresemundus Junior.*

"Remède Contre Fièvre Pestilencieuse" of [Jean Jacme], [1485]

Jean Jacme (Johannes Jacobi), who died in 1384, wrote a popular tract on the pest, which was often reprinted in the fifteenth century, in both Latin and French. The first Latin edition (Klebs-Sudhoff No. 62) is noted above. An undated French edition (Fig. 5), the Martini copy, also in the Boston Medical Library, was probably printed in Paris by Uldaricus Gering in 1485 (Klebs 543.1). It describes the plague of 1348, the same epidemic which caused the death of Gentile da Foligno. An edition of the Foligno tract is in the Boston Medical Library (Klebs-Sudhoff No. 51).

"De Pestilentia Signis, Causis et Remediis" of Bernardinus de Bustis, [1498]

A pest tract, unrecorded by Klebs, is found in the *Rosarium sermonum* of Bernardinus de Bustis (G. W. 5: 5807; B. M. C. v: 487). This sermon is of medical interest and should not be confused with other sermons, called *Judicio pestilentie,* which are entirely religious in character, having to do with sins, not pest.

[7] Catalogue of books printed in the XVth century now in the British Museum. London: 1935. 6:760.

Remede treſutile ꝓtre fieure peſtilẽcieuſe et autre maniere de eſpidimie aprouue par pluſieurs docteurs en medicine.

A Lonneur et a la louẽge de la treſſaincte et ĩdiuiſee trinite. de la glorieuſe et treſſacree marie mere de dieu. et auſſi de toute la court celeſtielle. Pour la ꝓſeruatiõ de ceulx qui ont ſancte et reformation des malades. ie veul ſoubz la correction de mes anciẽs maiſtres et docteurs aucunes choſes traicter et deſclairer ala choſe publique prouſitable. etcõtre la peſtilẽce qui ſouuẽt les corps humains ĩuade et ꝓtrarie. Les quelles choſes ie traicteray par ordre ſelon ma petite puiſſance en la maniere qui enſuit.

¶ Premierement

Des ſignes prenoſticables dicelle peſtilence C.i.
Des cauſes dicelle. C.ii.
Des remedes alencontre. C.iii.
De la ꝓfortation du cueur et ꝓncipaulx mẽbres. C.iiii.
De flebotomie. C.v.

¶ Des ſignes prenoſticables de peſtilence. C.i.

¶ Les ſignes par les quelz on peult ꝓnoſtiquer parler et cõgnoiſtre de la maladie peſtilencieuſe. ſont pour ce ꝓſent eſcripture aſſignes ſept tant ſeulement.

¶ Le premier ſigne eſt quant enuyron meſmes iour du temps deſte le vent ſe change et mue par pluſieurs fois tout ainſi q̃ ſe au matin il apert pluuieulx et apres obſcur et nubileux. et finablemẽt plain de vent. La quelle choſe ꝓncipalemẽt ꝓcede du vent meridional. ceſt a dire de midy.

¶ Le ſecond ſigne eſt quãt ſouuẽteſfois au temps deſte les iours ſaparoiſſent et ſe monſtrent totalemẽt obſcurs. Tout en telle maniere que ſe plouuoir il deuſt et non obſtant il ne pleut point. La quelle diſpoſition eſt a craindre. et ſigne de grant peſtilẽce quãt le temps demeure lõguemẽt en tel eſtat

¶ Le tiers ſigne eſt quant nous voions ſus la terre au temps deſte abundance de mouches. Et ce ſigne denote infectiõ de

Fig. 5.

The first page of the *Remède contre fièvre pestilencieuse* of [Jean Jacmel], [1485].

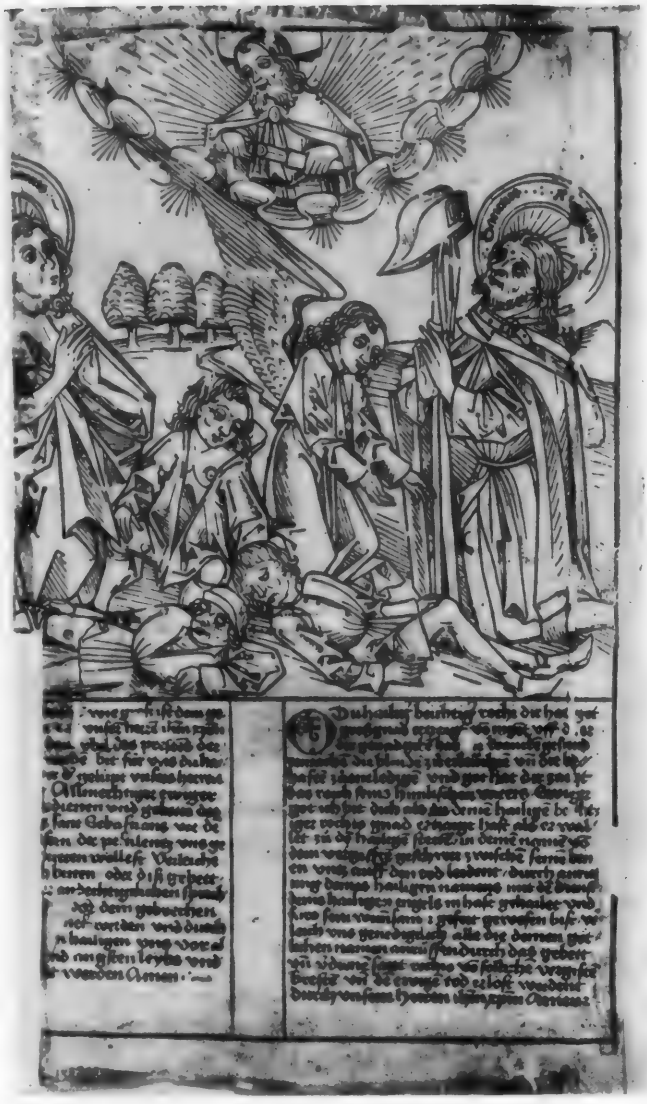

Fig. 6.

"Augsburger Pestblatt," ca. 1473.

The "Augsburger Pestblatt," ca. 1473

Broadside sheets were frequently printed during the fifteenth century as an easy means of disseminating information to the common people. Examples of the perishable leaves are rarely found. They were usually almanacs for bloodletting, descriptions of monstrosities, or supplications against syphilis or pest. An example of the last type is in the Boston Medical Library. This *Pestblatt* (Fig. 6) was described and studied by Weinberger,[8] who gives the opinion that it was probably printed in Augsburg about 1473, in relation to the epidemic of plague in that city, 1472-4.

The broadside has a considerable portion cut off the left margin, cutting into the figure of S. Sebastian and the prayer below the illustration. It is, nevertheless, a large sheet, 401 by 245 mm., probably too large to be considered as a book illustration.

The unusually clear engraving shows the plague-healing Saints, Sebastian and Roch, kneeling before Jehovah, who is seen in the clouds, drawing his sword for the protection of the sick. Beside S. Roch is his attribute, the comforting angel; at the feet of the Saints are three plague-stricken sufferers. Below the illustration are two columns of xylographic text. The left column is a prayer to S. Sebastian, met with in many plague leaves. It agrees closely in wording with a script described by Heitz and Schreiber.[9] The prayer on the right is to S. Roch and appears to be, according to Weinberger, previously unknown.

[8] Weinberger, Martin. Ein Augsburger Pestblatt. *Beiträge zur Forschung. Studien aus dem Antiquariat Jacques Rosenthal.* Neue Folge IV. München: 1932. Pp. 1-6.

[9] Heitz, Paul and W. L. Schreiber. Pestblatter des XV Jahrhundrets. Strassburg: 1901. No. 19.

"APOLOGIA" FROM AN UNFINISHED BIBLIOGRAPHY OF VESALIUS

By the late Harvey Cushing

With an introductory note by W. W. Francis

Introductory

Harvey Cushing had intended to contribute something to this special number in honor of his old friend, Dr. Klebs, the sort of friend who went to Oxford to see him get his honorary degree in 1938 and could cross the ocean to attend his 70th birthday party last April. To Cushing, as to many others of us, Dr. Klebs has for years been a wise counsellor and a ready help in time of bibliographic trouble. Witness these extracts from a letter Cushing wrote him as long ago as 1916: "Every time I get out my Vesal things I am surprised at the unending number of them and, what is more, much gratified to realize how many items have been added from time to time by your own good self"; then, after detailing some intricate difficulties about the extant title-page sketches, "You will clarify all this for me. . . . How I envy you your time and ability to dig out these things while I must dig things out of other people's brains instead of my own."

After crowning his great career with the publication of his "Meningiomas" in 1938, Cushing again "got out his Vesal things," this time in earnest and commandeering the family dining-room, with a view to a much needed bio-bibliography of his favorite hero. This has been left in the proverbial stage of "well begun, half done," and his manuscript and notes have been entrusted to me in the hope that the work may be completed. As there was no trace of his having started on his contribution to the Klebs Festschrift, his literary executors have suggested that something from the projected work, the opening chapter which he calls his "Apologia," might be acceptable.

And the rest of it? At his birthday party Cushing spoke of the worry which his "Meningiomas" had cost him: "I warn you against ever making any such mistake. Nothing is so devastating to one's peace of mind as a promissory note left unpaid." Thanks, Harvey, for the warning; I make no promises.

W. W. F.

APOLOGIA

The effective, moving, vitalizing work of the world
is done between the ages of twenty-five and forty.

W. O.

An incursion into bibliography has been compared to entering a dark forest full of unsuspected brambles in which even those with experience are prone to become entangled. One must not only step with care but have qualities of precision and alertness rarely possessed by those no longer young. Conrad Gesner, whom Osler so admired that he put him in his Bibliotheca Prima chiefly because he was " the father of bibliography," was only twenty-five when he began to collect the materials for his famous *Bibliotheca universalis* which was printed in 1545 by Chr. Froschouer in his native Zurich. Under the caption " Andreas Vesalius " on leaf 42 of this voluminous work, Gesner records all of Vesalius's publications that were then known to him and he ends his citation of the 1543 edition of the *Fabrica* by calling attention to the author's age as follows: " Scripsit autem Vesalius hos libros natus annos 28." As Gesner at the time was only 29 years old himself, he was naturally impelled to emphasize the age of the writer of this epochal work though far too modest to draw any further comparison.

When my personal interest in Vesalius was first aroused I am unable definitely to recall. It must have been nearly 40 years ago, for I find that I came into possession of Moritz Roth's remarkable biography in 1900 which indicates that there must have been talk of the man probably at the Johns Hopkins Historical Club prior to that time. This may have been what led my colleague W. G. MacCallum, after a year of study abroad, to lug home and present to me in the autumn of 1903 an imperfect copy of the 1543 *Fabrica* which he had come upon that summer among some books for sale in the back of a blacksmith's shop (of all places!) in a side street in Rome.

That same summer Osler as usual had been brain-dusting abroad whence came the usual shower of postcards to the Baltimore stay-at-homes. On one of them postmarked Guernsey, July 17th, he exclaims: " I have bagged two 1543 Fabricas! 'Tis not a work to be left on the shelves of a bookseller." And again from the same

place on July 25th: "Besides the two '43 Fabricas I have just ordered a third. We cannot have too many copies in America & no medical library is complete without one. We are having a nice quiet time here. . . ."

The upshot was that several copies of this historically important work were spread for comparison on his dining-room table one evening not long after the opening of the winter semester. The best of them he promptly inscribed as a wedding present for L. F. Barker. The next best he swapped with me for the MacCallum copy. A third (now in the Medical Library of the University of Missouri) was sent to W. J. Calvert, a recent graduate who had just been appointed Professor of Pathology at Columbia, Mo. This left him with the MacCallum copy and possibly two others (if there really were six) and his efforts to dispose of them are amusingly told in the introduction to his *Bibliotheca Osleriana,* as follows:

> For some years Dr. Harvey Cushing * and I had bought everything of Vesalius that was offered. One evening we had six copies of the first edition (1543) on exhibition. With the cash in pocket the book is impossible to resist, and I have distributed six copies to libraries. Forgetting what I had done, I took out a copy in 1907 to McGill, and showed it with pride to Dr. Shepherd, the librarian, who pointed out in one of the show-cases a very much better example presented by me some years before! Thinking it would be a very acceptable present to the Boston Library Association (in which I had a personal interest through Dr. James Chadwick and Dr. E. H. Brigham), I took the volume to Dr. Farlow, who looked a bit puzzled and amused. "Come upstairs," he said; and there in a case in the Holmes room, spread open at the splendid title-page, was the 1543 edition and, on a card beneath it, "The gift of Dr. Osler." I had better luck at New York, where the volume [1] found a resting-place in the Library of the Academy of Medicine.

Thus in the happy bygone Baltimore days those fortunate enough to come as latch-keyers under Osler's neighborly influence became inoculated with his bibliophilic spirit which has proved a solace for many of us in our later years even when it may not have developed into a hobby. Ere long a Vesalian club was organized, and though it was short-lived, an impromptu talk was given at its first meet-

* By way of protest H.C. in copying this quotation omitted his name. W. W. F.

[1] Dr. Malloch informs me that on one of the foreleaves is written "Medical Faculty, McGill College, from Wm. Osler October 29, 1903, the original edition of the greatest medical work ever printed."

ing by Dr. Welch on the man and his works. This is well remembered, not only for what he said, but because a few days later Howard A. Kelly left at my door with his card a superb copy from his own library of the 1555 *Fabrica* in contemporary stamped pigskin with clasps. So in those early days historically significant books might find their way to one's shelves, even though with an instructor's salary of $100 per annum there might be cobwebs in the purse.

In the effort that will follow to place the published writings of Vesalius in proper relation to those episodes of his life of which there is contemporary record, I am fully aware that to the perspicacious symptoms of the slowly progressive though rarely fatal malady once vividly described by Thomas Frognall Dibdin will be disclosed. Dibdin, however, failed to point out that the Bibliomania, while more commonly a masculine ailment, easily leads to difficulties because of an even more serious counter-disorder which is prone to affect the distaff members of the family, who usually control the majority vote. They show an uncontrollable tendency to accumulate odds and ends that go under the general name of antiques which mysteriously find their way into the house late in the day by way of the garage after all identification tags have been removed. On the other hand a new old-book, unmistakable in its brown paper parcel and bearing unfamiliar foreign stamps, is handed in openly at the front door by the postman, who must wait while the maid rummages round to find two cents for excess postage, thereby advertising its arrival from parlor to kitchen. Thus, while the advantages, as is quite proper, are all on the side of the antiquing distaffers, most collectors of books, in self defense, after obliterating from their recent acquisitions such highly interesting records of provenance as pertain to their source of origin and purchase price, conceal them on shelves in their so-called dens, fully convinced that they have made a better investment for the long pull than has the party of the first part.

To provide an example of this, my first actual purchase of a Vesalian item from a bookseller's catalogue was made in 1905 from Boas of Berlin for 18 marks. It was a much used and annotated copy, bound in green vellum, of Vesalius's first publication—his graduation thesis at Louvain. While it was not the first issue of

this tract, I have never but once since then seen another copy quoted. Of the first printing, so far as I am aware, but three copies survive only one of which is in private hands. Because of this windfall, as I am bound to confess, I was lured twenty-five years later into the purchase of a rubricated fourteenth-century Rhazes manuscript which presumably came from one of the Austrian ducal libraries many of which were being sold out at about that time. My tempter, the late Wilfred M. Voynich, wrote to say: "It is interesting to recall that in the fifteenth century Eberhard de Wesalia, the grandfather of Vesalius, wrote commentaries on Rhazes which Vesalius had before him while working on his *Paraphrasis in nonum librum Rhazae*. Since Eberhard became a doctor in 1433 and died young in his thirty-sixth year, i. e. before the invention of printing, it follows that he was obliged to study the works of Rhazes from manuscripts. While I do not wish to imply that he ever had the present one in his possession, however, in view of the fact that it contains commentaries in various hands, some of which date undoubtedly from the early fifteenth century, one cannot overlook the possibility that Eberhard, etc., etc. . . ." That proved an irresistable bait, though Mr. V. had conveniently forgotten just how the rare Rhazes MS. happened to come into his hands, even as I conveniently am obliged to forget just what was paid for it since manuscripts do not concern us here. It merely serves to show how one can be led by the nose from one thing to another.

Long before this concrete evidence of the fact that my resistance had completely broken down, I had begun to follow the footsteps of Vesalius and to seek traces of him in libraries, universities and art galleries whenever chance took me abroad, not to mention the perusal of auction sale-catalogues when at home. Notable among these sales was that of May 1911 when the library of Dr. Van den Corput (of Brussels) was auctioned at Muller's in Amsterdam, and when at a surprisingly low price excellent copies of the 1539 Venesection Epistle and—the tallest recorded one—of the 1546 China Root Epistle, along with twenty-five other items, were secured. A Vesalian autograph letter for which Osler and I had submitted a combined bid was withdrawn from the sale as its genuineness had come under suspicion.

In August 1909 Sir John Stirling Maxwell at Osler's request

sent his father's copy of the *Tabulae sex* to the Bodleian, where in the " Selden End " snapshots were taken of the separate sheets while the Regius Professor held the volume open. Curiously enough, L. F. Barker to whom that " best " copy of the 1543 *Fabrica* had been given six years before, happened to drop in, so Osler and his Baltimore successor were taken together flanking the exposed picture showing the venous circulation *à la* Galen. It was learned at the time that not all of the thirty copies of the facsimile edition printed in 1874 for Sir William Maxwell had been distributed, and Osler saw to it that suitable habitats for the remainders were suggested, which accounts for the one in my possession.

During a short sojourn in Munich, on the way to the International Medical Congress at Budapest in that same year, fairly good copies of the 1543 *Epitome*, both in Latin and in German, were purchased at a modest price from Ludwig Rosenthal. Later at Basel the Vesalianum was visited and I was permitted by the curator to mount a ladder and photograph in its glass case the skeleton with bifid xiphoid process that Vesalius had prepared with his own hands while awaiting proof sheets of the *Fabrica* as they were being struck off from the press of Oporinus. While now incomplete, lacking the mandible and lower extremities, it is the oldest anatomical specimen with known provenance that has been preserved, and above it in another sealed glass case are the skeletons of a female child and of an ape that thirty years later were prepared by Felix Platter, then the regularly appointed professor of anatomy at Basel. The tablet on the wall above the cases bears in letters of gold on a dark green background this inscription: " Skeletorum A. Vesalii nec non F. Plateri opera Basileae annis p. Ch. n. 1543 et 1573 paratorum quae supersunt."

The Scuola del Bo, colloquially known as the " Bo ", at Padua was visited more than once though there is not much now to see save Harvey's stemma and the old precipitate oval well of Alessandro Benedetti's anatomical theatre which was completed in its present form under Fra Paolo Sarpi's supervision while Fabrizio d'Acquapendente was professor. Vesalius would doubtless have been glad enough to have had such a room for his demonstrations though it was scarcely to be compared with the idealized auditorium shown in Calcar's frontispiece, which probably was intended to represent the

dignified surroundings suitable, in the opinion of Vesalius, for such important events as public anatomies. What his own makeshift quarters were probably like between 1537 and 1543 we can perhaps imagine from the account of the dissections held in Bologna in a temporary shed which the auditors finally had to abandon late at night because of the cold. Two hundred years passed before Bologna could boast of an anatomical theatre worthy of the name, but even that which Ercole Lelli finally saw erected would scarcely have held the 500 onlookers that are said to have crowded around Vesalius during his demonstrations in the heyday of his fame.

There seems to be no possibility today of identifying at Padua either the house of Count Gabriel of Ortenburg where he wrote the epistle on bloodletting in 1539 or the Casa degli Valli (called "Cavalle" in the 16th century) where for eight months he and the Englishman John Caius lived together, nor even the Ponte della Paglia near which it was supposed to have been located. Save for the other complete set of the *Tabulae sex* which I have twice had an opportunity to study in the Biblioteca Nazionale Marciana, there is little to be found today in Venice, though one would like to know where Titian had his school, where Calcar lived, and where was the printing house of B. Vitalis.

I had long been accustomed to carry about a folder in which the muscle figures from the second book of the Fabrica, extracted from the back of Moritz Roth's biography, had been mounted serially so as to show their consecutive landscape-background to which Jackschath in 1903 had first called attention. Professor Putti of Bologna had assured me that the region was familiar to him and could be easily found, but year after year we kept postponing our intention of trying to identify it. In 1932, however, Dr. Willy Wiegand, the Munich printer, was prompted to look into the matter and he succeeded in definitely locating the site as the region around Abano Terme, now a fashionable watering place in the Euganean Hills a short distance south by west of Padua—the countryside of Petrarch, as a matter of fact. There the site of the old Roman Thermae shown in ruins in Calcar's sketch, the Bacchiglione river with the bridge over it, and the rugged trachytic rocks can all be easily identified. One can imagine Calcar making his way there on a free afternoon and amusing himself while resting on a hill by sketching,

after the manner of Titian, the landscape panorama which he subsequently cut up and used as a background for the muscle figures.

Certain similarly alluring plans that were concocted were likewise postponed, I regret to say, until it was too late for them to be fulfilled. One was to trace the English ancestry of Elizabeth Crabb, the mother of Vesalius, a task which the late Sir Walter Fletcher always insisted should present no great difficulty.

An opportunity came in the summer of 1912 to pay an all too brief visit to Brussels which, however, made possible a full day at Louvain. There on entering the ancient portal of " Les Halles universitaires " one passed beneath old stone arches to a broad flight of much worn stone steps which led up to the main floor where was the famous library. In the " Salle des promotions " nearby I had the fortunate experience of hearing two young medical aspirants defend in Latin before a board of examiners their graduation theses, much as Vesalius in February 1537 must have done. Subsequently in the library I was shown a copy of the rare Louvain edition of his *Paraphrasis* and also the vellum copy of the 1543 Epitome in which the figures of the last two sheets had been cut and superimposed according to the author's directions. Could I possibly have foreseen that in another two years the building would be in ruins, I would have taken more snapshots and made more careful notes of the visit.

Preparations by this time were already on foot to celebrate the four hundredth anniversary of Vesalius's birth by appropriate ceremonies to be held at Brussels in December 1914, together with the publication of a memorial volume of essays. For this occasion Osler had persuaded me to submit a study of the supposed portraits of Vesalius scattered through European museums, copies of which I had long been collecting and only eleven of which, most of them ascribed to Titian, had been enumerated in 1892 by Moritz Roth. It was fortunately learned, however, that M. H. Spielmann, the Shakespeare iconographist, had been requested by Professor Heger, who had the program in hand, to undertake this contribution. Indeed before the outbreak of the war his illustrations and manuscript had been forwarded to Brussels and the photographs sent into Germany to be reproduced. Fortunately he had preserved duplicates of all his materials and long after the war, in spite of many vicissitudes, his

incomparable *Iconography* was finally published under the auspices of the Wellcome Museum in 1925. Many more posthumous portraits have subsequently turned up, most of them unquestionably based on the Calcar woodcut.

Personally I have a strong prejudice in favor of the portrait purchased for £39 by William Hunter at the sale in March 1754 of Richard Mead's effects, when it was specifically attributed to Titian. It is now in the Hunterian Collection at Glasgow, and though Mr. Spielmann passes over it lightly, it appears to me, taken in connection with the Glasgow Codex, to be the most likely contemporary portrait by Calcar in existence. It is beautifully painted and of course in the Titian manner which Calcar had learned skilfully to imitate. My second choice, for reasons to follow, would be the portrait in the Palazzo Pitti in Florence—of an older man, to be sure, with a heavier beard which is turning grey. He holds under his left arm a book which from its dimensions might well be a copy of the *Fabrica*. There could be no claim in this instance for Calcar who must have been long since dead, for this represents a man nearer 50 than 40 and if actually by the hand of Titian, as the catalogue states, it must either have been painted during some visit of Vesalius to Venice, or he conceivably might have sat for it at Augsburg where in 1550 Titian painted the portrait of Philip II that was sent to England as a lure—if the Habsburg chin could be so described—for Queen Mary. On my first visit to Florence, in 1909, 200 lire was all that a professional copyist who was at work in the gallery asked for making the copy which I still possess.

In following the footsteps of Vesalius one cannot escape from the shadow of Calcar, so closely are their reputations intertwined. In the University Library in Munich one may still see the original wood blocks which at the end of their peregrinations from Basel in 1555 to Augsburg, to Ingolstadt, to Landshut, have finally come to rest there; and elsewhere in Bavaria one may find occasional *objets d'art* based on one or another of Calcar's drawings. There is, for example, the tomb of the physician behind the altar in the Stadtpfarrkirche at Landsberg marked by a life-size skeleton, cut in stone, in the position of Calcar's contemplative figure; also the carving on wood, designed after the portrait sketch and discovered in 1913 by

Dr. A. C. Klebs, which decorates the façade of the Lemgoer Ratsapotheke.

As December 1914 approached my friend Dr. E. C. Streeter, who had a far better collection of books than I, proposed that we call attention to the date by an exhibit of Vesaliana for the spring meeting of the American Medical Association in Atlantic City. For this purpose we were allotted a booth and had a small pamphlet printed with a description of the books we had selected for display. It aroused little if any interest. We had arranged to alternate as showmen, Streeter in the morning hours, I in the afternoon. We met at lunch after the first morning and on my enquiring "How did it go?" he replied, "Well, only one old codger stopped long enough to read the sign VESALIAN EXHIBIT and said, 'Got any samples?'" Streeter asked "Samples of what?" He pointed to the sign and said, "Samples of Vaseline, of course." Sadder and wiser we returned to Boston with our "samples" at the end of the week.

The Brussels celebration was necessarily abandoned, but fortunately there was salvaged from the wreckage of those days a number of papers by Dutch physicians which appeared in the December 1914 issue of *Janus*. Among these was the notable Bibliography by Dr. F. M. G. de Feyfer of Geldermalsen which has been of the greatest possible service to the collectors of Vesaliana. After this invaluable contribution, together with Roth's scholarly and thoroughly documented Biography and Spielmann's notable Iconography, it would seem as though little could remain for anyone to say. It is, therefore, not with the vain expectation of improving on these three works that the present task has been undertaken, but rather with the idea that on their foundations a story of somewhat different sort might be strung together that would perhaps justify the effort.

I cannot leave this prefatory note without paying some sort of tribute to my many friends, past and present, in the book trade. I have already spoken of the late W. M. Voynich, to whom I owe much for his friendly advice about the purchase of books—not always to his own advantage. He had a scholar's love for books and usually read those in his possession, so that the biographical descriptions accompanying his sales were always both accurate and historically informing. The fine 1545 Geminus among my Vesaliana I owe to him; for knowing it would be snapped up quickly, he put it

aside on the grounds that some day perhaps I could afford it and he meanwhile could afford to wait.

There was another old friend—that bearded Scot, L. J. Syme, whom I have had reason to bless while composing this book. I first stumbled upon him by accident one hot August afternoon nearly forty years ago during a forlorn six weeks' sojourn in Paris visiting hospitals in the morning, studying French the rest of the time, and sleeping in a room *au troisième* under the roof of a boarding house at 67 rue des Saints Pères. He was sitting in a rocking chair on the sidewalk before his shop in the rue des Beaux Arts indifferent to everything but his pipe—indifferent even to the chance purchaser, whom he curtly (in my case) told to come back tomorrow. Through the narrow door, which his chair blocked, one could see nothing but a tortuous passage between books piled to the ceiling. Having nothing better to do I stopped in the next afternoon on my way home, and he produced from under his chair seven slender volumes which in 1900, for it was then, meant to me nothing whatsoever, a fact he must promptly have realized. When I asked him what they were, he said: "Never mind about that, young man, you'll find out in due time, and when you do you'll have reason to thank L. J. Syme."

The seven thin books, unbound and untrimmed, were small folios each of which bore on the title-page the signature of "Franciscus Rossius Nöeus Chirurgus Parisiensis"[2] with his date of acquisition. They were: (1) the *Tabulae quatuor* of Loys Vassé, Paris, 1553; (2) Galen's *De ossibus,* translated by Balamius, Paris, 1549; (3) the *Vaesani cuiusdam calumniarum depulsio* of Jacobus Sylvius, Paris, 1551; (4) Ryff's *Anatomica omnium corporis partium descriptio,* Paris, 1545; (5) the *In Hippocratis et Galeni physiologiae partem anatomicam isagoge* of Jacobus Sylvius, Paris, 1555; (6) Galen's *De usu partium* translated by J. P. Crassus, Paris, Wechel, 1548; and (7) the *De musculorum dissectione ex Galeno,* also by Crassus, Paris, Gazellius, 1548. As Mr. Syme foretold, I *have* good reason to bless him, for they not only have been useful to me these past few weeks, but any one of them today would fetch far more than he asked for the lot.

[2] François Rosse des Noeux, *fl.* 1552-84.

THE MUNIMENT ROOM AT ST. BARTHOLOMEW'S HOSPITAL, LONDON

SIR D'ARCY POWER, K.B.E., F.R.C.S.
Keeper of the Hospital Archives
London, England

I begin my contribution to his Festgabe by congratulating Dr. Arnold C. Klebs on arriving at the age of three score and ten with undiminished faculties and health. May he retain both for many years to come and may he long retain the affection of his many friends of which this volume is the outward and visible sign.

Dr. Klebs is sufficiently interested in early medical books that he might like to know what had happened before the invention of printing: what documents the custodians of a large general hospital thought worthy of preservation and what was their present condition.

About four years ago the Governors of St. Bartholomew's Hospital asked me to examine the Hospital records, say in what state I found them and make suggestions for their continued preservation. A new system of central heating had been introduced and it was desired to know whether the alteration in the temperature of the muniment room was beneficial or detrimental to its contents.

The Hospital Deeds

It was known that there were a large number of documents, that some of them had been examined by Mr. W. H. Cross, the Clerk to the Hospital, about 1870, and that many had been consulted and photographed by Sir Norman Moore, M. D., physician to the hospital, when he was writing "The History of St. Bartholomew's Hospital" which was published in two volumes (by C. Arthur Pearson, London) in 1918. I was fortunate in securing the skilled help of Miss Gweneth Hutchings, D. Phil. Oxon (now Mrs. Whitteridge) and we set to work in the Muniment Room in the basement of the Great Hall which was built in 1734. The room was small, ill-lighted and ventilated by a little window. We found that the documents were in need of attention; some were folded in envelopes, each with a summary of the contents in the handwriting of Mr.

W. H. Cross; many were tied up in bundles, others were lying loose or had been packed at various times into cardboard boxes. There was no catalogue or index and no attempt at arrangement. We had, therefore, many joyful surprises, for a deed executed in the time of Henry VIII might lie next to one written when Edward III was king. Fortunately they had not often been consulted and they, with the attached seals, were in a surprisingly good state of preservation. They were not damp and they had not been injured by mice, vermin or fungus. The grime of years was upon them and at the end of a morning we looked like sweeps.

Each document was examined separately and just as it came to hand without any attempt at arrangement by time or place. The majority were in Latin, one or two in Norman French. Each was given a card to itself, was read and summarised in English. The names of the witnesses were recorded, the seals were described and a date or approximate date was given. 1700 documents were dealt with in this manner, and we stopped at the year 1546 when King Henry VIII gave the hospital a new constitution. The staff at the Record Office kindly repaired the deeds and seals in the comparatively few cases where it was necessary.

The earliest document possessed by the hospital is dated 1137, the second year of the rule of King Stephen. It is a grant from Rahere, the Prior of the Convent of St. Bartholomew in Smithfield to Hagno, clerk, of the church of St. Sepulchre, he paying fifty shillings a year, twenty-five shillings at Michaelmas and twenty-five shillings at Easter, for the benefit of the canons and of the poor in the hospital. Rahere was founder of the hospital and the first Prior and founder of the neighbouring convent of Augustinian Canons. Hagno succeeded him as the Second Prior. The deed is interesting because it has never left the hospital, because the two original seals are still attached to it and because it is in excellent condition, except that the rim of one seal is chipped .

Royal Charters

The Priory and Hospital soon acquired merit. Founded without any written document it was granted a charter by Henry I at some date between June 24th and August 2nd, 1133. The original deed

has disappeared, but a copy of it is in the Cartulary. It was confirmed more especially as regards the hospital by King Henry III, on June 15th, 1253, who states that " this Hospital with all that belongs to it I have taken into my hand and protection and defence against all men as my own chapel and I will that it should be as free from all earthly power and service as my crown." The grants, he adds, are made " for the love of God and my own welfare and that of my heirs, for the stability of the kingdom and for the soul of King Henry, my grandfather."

Early Benefactors

The Hospital, as became a religious foundation, soon made arrangements for a chapel and a cemetery. In the beginning, there was an altar in the only ward—a hall of considerable size with a fireplace in the centre. The altar was visible to the patients where they lay, but as early as 1147 it was ordered that the chapel in the middle of the hospital should be moved; that the entrance from the horse market should be closed and that the chapel of the Holy Cross should be completed. The site is now occupied by the Church of St. Bartholomew's-the-Less. The building of the church seems to have hung fire and it is not until 1183 that Pope Lucius III orders the oratory of the Hospital to be moved and commands the Bishop of London to consecrate the building without raising any objection. A later Bull of Pope Lucius III, bearing the date August 29th (1191), gives the hospital the right to have a graveyard of its own " on account of the number of deaths in the hospital and because of the distance of the nearest cemetery and the difficulty of access to it right across the horse market and the muddy streets. This increases the work of the brethren and servants attending the funerals and it has lately become very much worse." These statements were clearly incorrect to anyone who had personal knowledge for the hospital frontage had been given by the Bucointes in about 1167, it had been railed off and and there was only a single narrow street, now called Little Britain, to cross on the way to the Priory churchyard. But it was desirable to have a hospital cemetery as the burial fees brought grist to the mill when opulent citizens wished to be buried amongst the poor. The site chosen is known because Sir

Thomas Bodley was allowed to rent a portion of it 400 years later when he built the gallery to hold his treasures before they were moved to Oxford on the foundation of the Bodleian Library. Even then the mortuary chapel, dedicated to St. Nicholas, is shown on the plan of the property made about 1610.

There are many deeds showing that the citizens of London valued the works of the hospital. Great and powerful families like the Bucointes, the Blunts and the Ailwins were large benefactors during more than one generation: smaller men gave the rent of a house or a shop and in later days the more religious founded chantries and endowed a priest to serve them. The deeds show how much land came into possession of the hospital in different counties, Essex in particular. Here Robert Bruce (1210-1295) gave the hospital an acre of land in Little Wakering and he was Robert Bruce VI, the first chief justice of England " facetus, dives, et largus," the grandfather of Robert Bruce VIII (1274-1329), King of Scotland, of whom it was told that he learnt patience from the spider. Both the deeds of Robert Bruce the justiciar are in an excellent state of preservation with their seals still attached to the parchments.

The battle of Evesham was fought on Tuesday, August 4th, 1265 and King Henry III confiscated the property of " Thomas Fitz Thomas late Mayor of London, one of our enemies and rebels who adhered to Symon de Montfort, late Earl of Leicester, our enemy and felon." The charter was enrolled in the reign of Edward III on January 22nd, 1274, and passed into the hospital's possession in 1348 on the death of Joan Pertenhale—a professed sister in the hospital—who had inherited a part of the confiscated property.

It is interesting to note that in times of emergency like those of Simon de Montfort's Rebellion and the epidemic of bubonic plague in 1348 there are several documents which are wholly unconnected with the affairs of the hospital. It seems, therefore, as if the muniment room were sometimes used as a place where deeds could be stored in safety and, the owners having died or been killed, the heirs never reclaimed them.

The Cartulary

Most of the deeds are quitclaims dealing with property which had come into the possession of the hospital at various times. They had

been copied into a cartulary written by John Cok who was born in 1392, had been apprenticed to a goldsmith and became a brother of the hospital in 1421. As a brother it would be his duty to attend to the patients and later as " Renter " he copied the documents. He was alive " but in the evening of his life " in 1468. His accuracy as a copyist was great for we have compared most of the deeds he transcribed with the originals, finding very few errors either of omission or commission. He was a real Cockney for he loved a show and records as a little detail " In the year 1413 on the ninth of the month of April, which day was Passion Sunday and a very rainy day, the coronation of Henry V took place at Westminster, at which coronation I, Brother John Cok, who have recorded that royal coronation for the refreshing of memory, was present and beheld it." His handwriting is beautiful and his imagination was lively, as is shown by the little ornamentations of animals and plants with which he adorned his cartulary.

Papal Bulls

For the first two hundred and fifty years after the foundation there were frequent quarrels between the convent and the hospital, as might have been expected when there was dual control. The hospital was independent of the Priory which yet had some rights over it. Our documents tell of the causes of the quarrels which now appear to be extraordinarily trivial. The master and brethren of the hospital are to go in procession after the Prior and Canons of the convent: the hospital is not to set up an altar dedicated to St. Bartholomew nor may they have an image of him. The hospital is to have only two bells and they are not to be hung in a tower nor rung before the priory bell. This was in 1224 on July 24th and the convent seems to have won. One hundred and forty-nine years later the quarrels culminated in an agreement by which the hospital obtained most of what it wanted. The master might keep the official hospital seal: neither he nor the brethren need go in procession: the dead might be buried in the hospital graveyard: there were to be five bells hung in a tower and they might be rung whenever the hospital wished, but there was still to be no altar or image of St. Bartholomew. Still there were advantages in belonging to the hospital. During the interdict in 1208 when the whole kingdom was

excommunicated the Master and brethren had permission to celebrate mass with closed doors and in a low voice, taking care that no outsiders were present; they had an allowance of consecrated oil and were allowed to bury their dead.

Foundation of the Charterhouse in London

The deeds in connection with the sale of the land on which the London Charterhouse is built are singularly complete. The first is an indenture dated November 15th, 1370, witnessed by John Sernes then Mayor of London, William Walworth and Robert Kayton the sheriffs, who are called vicecomites.

The deed gives the thirteen acres and one rood of land formerly called the Spitalcroft but now Newchirchehawe near the Westsmithfelde bar at a rent of eight pounds a year to be paid in equal parts at Easter and Michaelmas by Sir Walter Manny, who gives twelve pence as seisin for greater security in the matter. The indenture is made between Stephen of Maydenheth, Master of Bartholomew's Hospital in Westsmythfelde in London and the brethren of the same hospital of the one part; dominus Walter de Manny, miles, dominus Richard de Evesham, clericus and John Whitewell of the other part. The date is November 15th, 1370.

The next document bears the date February 6th, 1371 and is a licence from Edward III to Walter dominus de Manny to found a Carthusian monastery on the land he had bought outside the West Smithfield bar. The monastery to have a prior and to be dedicated to the honor of God and the blessed Virgin Mary by the name of The Salutation of God. Its object to provide masses, prayers and other divine offices for the continuance in health " of ourselves, of the said Walter and of Margaret his wife whilst we are living and for our souls and the souls of our predecessors and of our heirs as well as for the souls of Walter and Margaret and of their ancestors and heirs when they shall have been taken from the light of day, and also for the souls of Alice de Henaud and of Michael of Northborough, lately Bishop of London, and of all Walter's benefactors and of all the faithful in the burial ground. This special permission is given notwithstanding the statute " de terris et tenementa ad manum mortuam." (The Mortmain Act which had been promulgated in 1279.)

The final deed is dated March 28th, 1372, and was sealed by Walter de Manny with, as witnesses, John Barnet, clericus: the Bishops of Lincoln and Salisbury, Lords Pembroke, Hertford and March, his son-in-law John Hastings, the Lord Mayor and sheriffs of London. It recites much of the King's Licence, states that permission for the monastery has been given by the motherhouse at Salandra, where the Carthusian order began, changes the name to "the Salutation of the mother of God" and appoints John de Lustote as the prior. It adds to the description of Alice de Henaud that she was formerly Countess Mareshall. It is not known how the attesting witnesses signed. Did they sign individually and affix their seals or were they all present and if so what a gathering it must have been, the Bishops in full canonicals representing the church: Manny's friends, the very flower of chivalry in its fullest blossom, old companions in arms whose names have come down to us in history and one—John Barnet—the clerk, lately Treasurer of England, Bishop of Ely signing not as Bishop but, perhaps, as representing the king himself. He had but a few more months of life vouchsafed him for he died in June 1373 and his tomb can still be seen on the south side of Ely Cathedral. The Mayor of London with both the sheriffs in testimony of the citizens' good will, though the gift lay outside their jurisdiction.

Of the sheriffs the senior, William Walworth, could not have foreseen that his name would become memorable not only in the history of London but also in that of England. He could not have foreseen that hot and eventful June 15th, 1381 when, as William Walworth, Mayor of London, he stood in West Smithfield with the boy king by his side facing an angry mob and watched Wat Tyler call "for a mug of beer, gargle and spit its contents filthily over the king." Cavendish, a young squire, saw the insult and avenged it by stabbing the fellow who was dragged through the hospital gate and there died.

The Duties of a Chantry Priest

The age was one of deep religious feeling and several chantries were founded in the hospital. One of the first was that established by William de Stowe in 1341. The Deed runs:—

"William, having obtained license in mortmain from King Edward, grants to the Master and Brethren of the Hospital two messuages and six shops in the parish of St. Sepulchre for the maintenance of two chaplains, brothers in the Hospital, to celebrate mass each day for the souls of William Stowe, his father, and mother, and kinsmen, and all the faithful departed. The two chaplains are to be brothers regular of the Order and to be maintained over and above the six regular chaplains kept in the Hospital. These two shall celebrate mass, with Placebo, Dirige, and Commendations in the Hospital Church. If one of the chaplains die, or be otherwise prevented from officiating, within a month of his decease, or inability to officiate, the Master and Brethren shall elect as successor a secular chaplain suitable to enter the order and shall nominate him within a month to the Prior and Convent of St. Bartholomew (or to the Sub-Prior, if the office of Prior happen to be vacant) who, within a fortnight of his nomination shall present him again to the Master and Brethren who, within the same fortnight, shall receive him into the order and rule of the Hospital as a brother and give him the religious habit worn in the Hospital. If the Master and Brethren neglect to nominate to the said Prior, or Sub-Prior, a suitable secular chaplain within the month aforesaid, then the Prior, or Sub-Prior, within the fortnight next ensuing, may present to the Master and Brethren who shall receive him as above rehearsed. If the Prior, or Sub-Prior, fail to do this, then the Dean and Chapter of St. Paul's, or the Chapter alone, shall present a suitable secular chaplain to the Master and Brethren, who shall receive him as above rehearsed.

Being admitted, the two chaplains shall celebrate each day in the following manner: One of them shall celebrate each day at dawn the mass for the day with the second collect for St. Mary. He shall say the third collect: "Deus cujus nomine non est munerus etc." for the souls of William de Stowe and Brother Richard de Eccleshale; the fourth "Inclina Domine etc." for the souls of Andrew (his father), Alice (his mother), and his brothers and sisters. The other chaplain shall celebrate the Mass for the dead with the principal collects for the souls of William, Andrew, Alice, his brothers and sisters, and Brother Richard and of all the faithful departed: "Deus

cujus nomine etc." He shall say the second collect: "Inclina Domine etc." also for their souls.

If one of the said chaplains be prevented from celebrating, then another of the Hospital chaplains shall be substituted to celebrate the full office for the dead with Placebo, Dirige, Commendations and the Mass which the said chaplain should say if he himself were celebrating.

The Master and Brethren shall each year on the anniversary of the death of William Stowe cause to be celebrated a solemn Mass with music, Placebo, Dirige and Commendations for the repose of his soul and the souls of the faithful departed.

All the above conditions shall be rehearsed in full chapter each year upon the anniversary of his decease, and all the promises within a month of the date of the present deed shall be written down in their calendar of benefactors. And the Master and Brethren shall swear to the Prior, or Sub-Prior, to observe all these things—and their successors shall do likewise. To this intent this indenture was made tripartite that one part might be kept by the Master and Brethren, the other by the Prior and Convent, and the third by the Dean and Chapter of St. Paul's—whose seals together with that of William Stowe—were affixed."

The vast number of documents deal with property acquired by the Hospital in the course of time. Each describes the property in detail, states where it is situated, and in London the names of the owners of the adjoining properties, the rent to be paid and the times at which it is due. Each deed bears the names and occupations of the witnesses and in very many instances the seals still remain in excellent preservation and with their original attachment.

Wills

The next group of deeds consist of wills and from them it is evident that bequests were made by people in every station of life. Most, of course, were made by men but here and there is one by a woman, for instance a daughter of a well-to-do citizen who had long served the hospital as a professed sister.

A notable feature of the fourteenth century wills is the quantity

of silver plate which appears to have been in daily use, flagons, chalices, tea-spoons, etc.

The earliest will now in possession of the hospital was made by Reginald de Wirham, about 1220, when he was setting out on a pilgrimage to the Holy Land, from which he appears never to have returned. It states that, out of the twenty shillings of rent which he had in the parish of St. Mary Somerset, and which he had purchased from the widow of a certain Alan Balun, he left ten to God and to the maintenance of a chaplain to celebrate the mass of the Blessed Virgin Mary forever in the Hospital of Saint Bartholomew. To the other hospitals of London he also makes bequests, but to none so large a gift as that to Saint Bartholomew's; to the hospital of St. James, half a crown from the same rent of twenty shillings; to the hospital of St. Giles, half a crown from the same rent. If Reginald die on the journey to Jerusalem, the hospital of St. Bartholomew shall have plenary seisin of the said rent, provided that the Master and brethren pay the lords of the fee the accustomed services and make sufficient guarantee to the other hospitals for the rents bequeathed to them. Bequests to other hospitals and religious institutions follow, with a clause guaranteeing, in the event of his death, plenary seisin of the rent to St. Thomas's Hospital, provided always that the legacies made out of this rent be rightly and duly administered. To his companion, Peter Burgensis, "if God grant him safe return from the Holy Land," he leaves his share in a shop in Cloth Fair, and his shop in Winchester Market, provided that Peter give sufficient guarantee to the Canons of St. Bartholomew for the payment to them of an annual rent of twelve pence. If he fail to return, these properties are to go to the Hospital of St. Bartholomew on the same conditions.

The will of Matilda de Kersing, made on St. Cecilia's day (22nd November) 1280, is chiefly concerned with her personal belongings, and has, therefore, a peculiar interest of its own. Also, it is rare to find a will made by a woman at this date, and its rarity enhances its value. Matilda died before November 15th, 1281, and the Archdeacon of London set his seal to the document allowing it valid on January 25th, 1282/3. The proving of wills seems to have been no more and no less tedious a process in the Middle Ages than to-day.

"In the name of the Father and of the Son and of the Holy Ghost, Amen, I, Matilda de Kersing, make my will on the day of the blessed Cecilia the Virgin in the year of Our Lord 1280. First, I leave my soul to God and to the Blessed Virgin Mary and all the saints, and my body to be buried in the cemetery of St. Andrew at Farnham. To the high altar, I leave the sum of six pence; to the vicar, six pence; to the chaplain of the parish, four pence; to the chaplain of the Blessed Mary, four pence; to Nicholas the chaplain, four pence; to the two clerks, four pence; (there legacies were probably made as an encouragement for the solemn celebration of her obsequies). To the light before the Blessed Virgin Mary, I leave twelve pence; to the brethren of Guldeford (? Guildford), five shillings and a black tapet, the best cloth which I have and the best towel; to the lepers of Awelton, a little tapet and a linen cloth. To Alice, daughter of Robert le Massun I leave a chest; to Margaret, wife of Robert le Massun, a brooch of the finest gold; to Beatrice, wife of John de Heghe, a russet tunic, my best cloak, and my best shoes. To Beatrice's daughter, a cloth and a towel, a quilt and a cushion; to Cecilia Pidekin, a shift; to Emma Botisse, a shift; to Cecilia Pidekin, a pair of shoes; to the Convent of Wynteneie [near Winchfield], the cup of the Blessed Edmund. For alms to be given on the day of my burial, I leave eight shillings. To John de Heghe, I leave a brass cauldron and a basin; to Cecilia Pidekyn, a little brass cauldron; to the son of William Carter, four pence; to Matilda, daughter of Robert Humfraye, a towel and a cushion.

To the Hospital and brethren of St. Bartholomew in Smithfield, London, I leave the thirteen and four pence of annual rent which the brethren of St. Thomas in Southwark were wont to pay me. To Beatrice, wife of Johne de Heghe, I leave a green hood. I make and constitute as the executors of this my will Richard le Massun and Robert, son of John Clerk. All the goods which are not named in this writing I leave to be distributed for the good of my soul in the parish of Farnham in whatsoever places my executors see they may be used to the greatest profit."

And so it is possible to continue the story to the present day, for the records are well kept and the journals are posted up continuously.

WALTER CHARLETON, D.M., F.R.C.P., F.R.S.

By HUMPHRY ROLLESTON
Haslemere, Surrey, England

Walter Charleton (1619-1707) was born on February 2, 1619/20, in the rectory at Shepton Mallet, Somerset, as the son of Walter Charleton, then rector, and previously vicar of Ilminster. The future physician was said to have made a sudden entry on a separate existence at 6 p. m. and surprised his mother at supper.

William Courten (1642-1702) the naturalist and one of the early benefactors to the Ashmolean Museum, Oxford, who for many years lived under the assumed name of William Charleton, was not a relative of the physician Walter, and probably the same is true about Rice Charleton (1710-1789) a physician of Bath and describer of its waters, and F. R. S., 1748.

Among medical authors in the seventeenth century Walter Charleton was one of the most versatile and prolific, but the appreciations of his activities are comparatively few, though he was noticed in the *Dictionary of National Biography* and in Munk's *Roll of the Royal College of Physicians of London*, and Frederic Manning gave a critical estimate of his philosophical and non-medical writings.

Educated by his father he entered as a commoner at Magdalen Hall, which became Hertford College, Oxford, in 1635. The history of Magdalen Hall goes back as far as 1282 when Elias de Hertford opened a hostel for students which was variously known as Hertford, Hert, or Hart Hall; it went through several transformations and for a considerable time was a dependency of Magdalen College, but in 1602 became independent. Charleton matriculated on 3 July in the Trinity term 1635 when 16 years old, then quite a usual age to begin life at the University. Here he came under the influence of John Wilkins (1614-1672) " the principall reviver of experimentall philosophy (*secundum mentem domini Baconi*)," then tutor of that house, and in 1648 as a parliamentarian became Warden of Wadham College, where the famous " Experimentall Philosophicall Clubbe " often met in his lodgings; it contained Seth Ward (1617-1689), William Petty (1623-1687), Thomas Willis (1621-1675), John Locke (1632-1704), Robert Boyle (1627-1691), William,

Viscount Brouncker, and other future fellows of the Royal Society founded in 1662, Wilkins being the chairman of the organizing committee and the first secretary of the Society. Wilkins had the exceptional experience of being appointed master of colleges in both Oxford and Cambridge, namely master of Trinity College, Cambridge in 1659, from which, however, he was ejected on the Restoration in the following year. From his early teaching Charleton " profited much beyond his years in logic and philosophy, but his genius soon after leading him to the study of physick, he in a short time made as great progress in that faculty as he had before in arts, and therefore by the favour of King Charles I was actually created doctor thereof on January 16th, 1642/3, and about that time made one of his physicians in ordinary " (Anthony Wood). Charleton's appointment at such an early stage has been regarded as rather a gracious act to a promising member of a loyal University than as a serious provision for medical emergencies, especially as William Harvey (1575-1657) was in attendance on Charles I from 1642 to 1646 at Oxford; but probably Harvey suggested or approved the appointment. As he was then making dissections and working on the material published in 1651 in his *Exercitationes de generatione animalium*, he may well have influenced his very junior colleague; the British Museum contains a copy of the *Disquisitio anatomica de formato foetu* (1667) by Walter Needham (1631-1691) with notes in Charleton's characteristic and beautiful script, thus showing his interest in the subject. Later, and after a respectful interval, in 1683 he followed Sir George Ent and his *Apologia pro circuitione sanguinis* (1641) in disproving the claim for priority in the discovery of the circulation of the blood made on behalf of P. P. Sarpi or Padre Paolo of Venice (1552-1623) after his death. This tribute to Harvey was paid in the first of " three anatomic lectures ": (i) the motion of the blood through the veins and arteries, (ii) the organic structure of the heart, and (iii) the efficient cause of the heart's pulsation, given on March 19, 20, 21, 1683, in the " anatomic theatre of His Majestie's College of Physicians, and published by command of the most learned president."

Like some scholar-physicians of his time Charleton was primarily a scholar, and only secondarily a medical man. While in Oxford from 1645 to 1650 he evidently read widely and intensively, espe-

cially the works of J. B. Van Helmont (1577-1644), the founder of the iatrochemical school, and wrote with corresponding energy, as shown by his output in 1650, the year he left Oxford: two translations from Van Helmont—*De magnetica vulnerum naturali et legitima curatione* * (1621) or *A ternary of paradoxes: the magnetick cure of wounds*; the dedicatory epistle to his patron Lord Brouncker contained information about the sympathetic cure of wounds communicated by Sir Kenelm Digby (1603-1666). The other translation was of *Deliramenta catarrhi; or The incongruities, impossibilities, and absurdities couched under the vulgar opinion of defluxions.* "The author, that great philosopher by fire, Joh. Bapt. Van Helmont: the translator and paraphrast Dr. Charleton, physician to the late King." In the same year and obviously inspired by *De Lithiasis* (1644) of Van Helmont, Charleton brought out *Gorgonicus, vi sua saxipara exutus, sive de causis, signis, et sanatione lithiaseos diatriba*, in which the formation of calculi was ascribed to a stone-forming spirit.

In 1650 Charleton left Oxford to settle in Russell Street, Covent Garden, to practice. He was then appointed a travelling physician to Charles II and physician in ordinary to the King while in exile, having as a colleague his friend Sir Edward Greaves (1608-1689), previously superior Linacre reader in physic (1643) and fellow of All Souls College, Oxford. He was the premier medical baronet, and appears to have been created on 4 May, 1645, at Oxford by Charles I; but as the baronetcy was conferred during the civil war it was never registered at the Heralds' College, and its validity has therefore been questioned. The office of travelling physician to the King in exile appears to have been both honorary, as there was no emolument, and nominal, for Charleton did not leave England. A constant Royalist he did not seek any favours from the Commonwealth and on the Restoration was confirmed in the post of physician in ordinary. In 1661 he published *A Character of his most sacred Majesty Charles II* in most laudatory terms, describing him as one to whom no interest was so dear as religion, which must have tickled the merry monarch's sense of humour.

At the Royal College of Physicians in London Charleton held

* Osler considered that in magnetic sympathy Van Helmont clearly expressed the doctrine of immunity and the cure of disease by immune sera.

many offices which present some incidental points of interest. In 1650 when he moved from Oxford to London he was on 8 April admitted a candidate, a stage of probation somewhat like that of the present membership, then usually for a year, before election as a fellow. But the times were much disturbed and it was not until 1664 that he became an honorary fellow. A short explanation may be given here about this new order of honorary fellows or socii honorarii; this information is given in Munk's *Roll* and in the manuscript but never published histories of the College by F. J. Farre (1804-1886), treasurer of the College (1868-1883), and also by W. Munk (1816-1898), Harveian librarian (1857-1898). During the Cromwellian era the affairs of the College had fallen into great disorder. Their funds were nearly if not quite exhausted. The lectures were suspended for want of money to pay for them, and a large number of physicians were settled and practising within the liberty but without the licence of the College, and the examination of apothecaries' apprentices, which in previous years had been rigorously enforced, had been discontinued. Sir Edward Alston (1595-1669) president of the College, who had been knighted by Charles II in 1660, bestirred himself and, with a view of bringing the physicians in London into the College and thus improving its finances, was the moving spirit in the initiation of the new order of fellows who without examination were given all the privileges outside but not inside the College. This was successful in bringing in 74 honorary fellows and £800 in December 1664. In 1674 Charles II forbade the order; in 1752 a motion to revive the order was negatived, and in 1780 an attempt at its revival was made but, as it demanded an examination, without much success. A new category of honorary fellows was instituted at the tercentenary (1928) of the publication of Harvey's *De motu cordis*, when Lord Balfour, Lord Rutherford, I. P. Pavlov, and K. F. Wenckebach were elected; the only election since was that of Harvey Cushing in 1939. The establishment of the new order of fellows which had promised so well was followed by misfortune; in 1665 during the plague when the president and most, if not all, of the College officers were seeking safety out of London, the College was broken into and the whole of the funds stored in the treasure chest were stolen, and in the next year the College, then in Knight Rider Street, and most of its library were destroyed in the great fire.

In 1676 Charleton became an ordinary fellow of the College, probably because as an honorary fellow he was without the privileges of an ordinary fellow. His activities inside the College soon became prominent; he was censor in 1677, 1682, and senior censor from 1698 to 1706; he gave the first lecture, on anatomy, in the Cutlerian theatre of the new College in Warwick Lane in 1680; delivered the Harveian oration in 1680, 1702, and 1705 (the last fellow to give it more than once was Sir Henry Halford in 1800 and 1835); was "elect" 1686-1693 and 1701; consilarius in 1687 and from 1702 to 1706; president 1689, 1690, and 1691; and was elected Harveian librarian with a salary of £20 a year on 6 December 1706.

On 20 May 1663 Charleton was elected an original fellow of the Royal Society, at the same time as a number of his friends, such as Lord Brouncker, Henry Marquis of Dorchester, and Sir George Ent. He was much in evidence at the meetings. Thus with Robert Hooke (1635-1703) he carried out experiments on grafting the skin from one part to another of a dog (1663) and the spur to the head of a cock, and was nominated to investigate the velocity of a bullet shot out of a musket (1664). It has been stated that in 1678 he was offered and at first accepted and then declined a chair of practical medicine at Padua (*Dictionaire des sciences médicales*).

Charleton's portrait, painted and engraved in 1679 by David Loggan (1635-1700?), is in the Royal College of Physicians of London and was reproduced in his *Enquiries into human nature* (1680). An earlier portrait was inserted in his *Immortality of the human soul* (1657).

Soon after he ceased to be president of the Royal College of Physicians Charleton who had outlived most of his old Royalist patients became so much embarrassed by the lack of riches that he disappeared from London for some years, first to Jersey and then to Nantwich. He thus shared the same misfortune as other ageing physicians, especially that of his senior, Thomas Grent (? 1584-1649) to whose widow the College extended a helping hand. It seems probable that a similar course was adopted towards Charleton when he returned to London about 1698, by the number of College offices conferred on him until his death, after a lingering illness, on 24 April 1707. He was described by his contemporary Anthony Wood (1632-1695) as "a learned and an unhappy man, aged and

grave, yet too much given to romances." By his own admission (*Immortality of the human soul*, 1657) he was subject to depression, and by " romances " Wood was probably referring to his love for speculation.

Some reference may be made to his friends. Thomas Hobbes (1588-1679), the philosopher and author of *Leviathan* (1651), though he preferred " an experienced old woman to the most learned but inexperienced physician," had a high opinion of his abilities and exerted a considerable influence on him. In Charleton's translation of Van Helmont's *Deliramenta catarrhi* (1650) the preliminary section " the translator to the judicious and (therefore) unprejudicate reader " refers to " that noble enquirer into truth, Mr. Hobbs and his inestimable manual *Human nature* " (1650). Hobbes probably directed Charleton's attention to the writings of Petrus Gassendi (1592-1655) the philosopher and reviver of Epicureanism in France, and thus played an indirect part in the production of *Physiologia Epicuro-Gassendo-Charletoniana; or a fabrick of science natural, upon the hypothesis of atoms, founded by Epicurus, repaired by Petrus Gassendus, augmented by Walter Charleton* (*1654*); this was a treatise on Epicurean physics, the word " physiology " being used in its now obsolete sense of natural science in general. Charleton and Hobbes had a good deal in common; Hobbes was antagonistic to Descartes, and Manning spoke of Charleton as alternating between the rival schools of Descartes and Gassendi, his natural bias being towards Epicureanism and away from the stoic element in Descartes. In 1656 he produced under the title *Epicurus his morals* a translation of fragments collected from various sources, and not all from Epicurus.

William Viscount Brouncker of Castle Lyons (? 1620-1684), mathematician, first president of the Royal Society (1662-1677) and created doctor of medicine at Oxford on 23 February 1646/7, was his patron and generous friend, and cast his horoscope. In several of Charleton's dedications to " persons of honour " or " eminent persons " who were left anonymous it may well be that Brouncker was meant.

Henry Pierrepont, first Marquis of Dorchester and second Earl of Kingston upon Hull (1606-1680) was a most generous benefactor to the Royal College of Physicians in London, to which he

Fig. 1.

Portrait of Walter Charleton about 1657.

Fig. 2.

Portrait of Charleton in 1679.

left a library then valued at £4000 and of which he was elected a fellow in 1658. He regarded medicine as his most serious accomplishment, but was ridiculed by his contemporaries for an assumption of universal knowledge, just as two centuries later it was said of William Whewell (1794-1866), the famous master of Trinity College, Cambridge: "Knowledge his forte and omniscience his foible." Dorchester's portrait was painted at the expense of the College as is shown by the note from Charleton as president to Thomas Burwell (1626-1702) then treasurer of the College (see Fig. 3).

Sir Francis Prujean (1593-1666) and Sir George Ent (1604-1689) were his seniors, Cambridge men and, like him, presidents of the Royal College of Physicians. Except that he was also the son of a clergymen, Prujean presented a contrast to Charleton; a successful Court physician he died very rich, was "no great painstaker," but was of very great judgment, married twice, never wrote any published work, but showed John Evelyn his laboratory, and played to him on the "polythore, an instrument with something of the harp, lute, and theorbo, but known to none in England but this skilful and learned physician" (quoted by Munk). The epistola dedicatoria of Charleton's *Darkness of Atheism dispelled by the light of nature*, a physico-theological treatise, was headed Clarissimo quia ornatissimo viro Franc. Prujean. Ent was much closer in age, tastes, and activities to Charleton who dedicated two of his numerous works to Ent (a) (in company with Thomas Viscount Fauconberg) *Exercitationes physico-anatomicae de Oeconomia animali* (1659), and (b) *De Scorbuto liber singularis*, 1672, with inscription to the then president of the College "amplissimo consultissimoque Georgio Ent, M. D., Equito Avrato. Ent both wrote and lectured, and when giving the anatomy lectures at the College in 1665 was knighted by Charles II, the only occasion on which such an honour has been conferred within its walls.

Charleton's Writings

The bibliography of Charleton's numerous writings has presented difficulties in the past; Munk gave a list of 25 which he feared was incomplete; Manning, who described a bibliography as "the ritual form of acknowledging the books one has not read," accumulated 27

items, and Norman Moore listed 30 printed works and four in manuscript. Among the difficulties are the number of various editions in English and in Latin, not infrequently with some alteration of title, of seeing them all in an available library, and the amount of repeti-

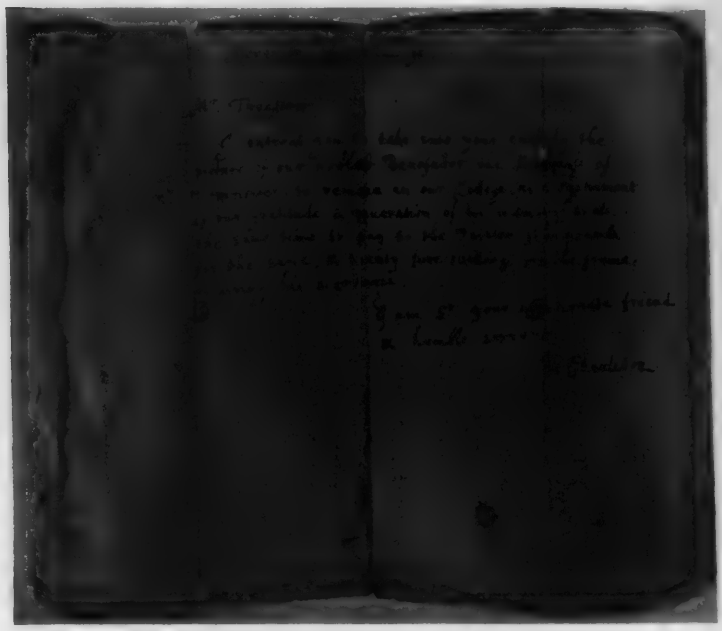

Fig. 3.

Photograph of Charleton's Script.

tion of the same subject in his various publications such as the *Exercitationes physico-anatomicae*. The subjects of his pen cover a wide field—religion, philosophy, physics, physiology, zoology, and psychology. Reference has already been made to some of his works, some short notes on other subjects follow, and then a chronological list has been compiled from various sources, especially from Norman Moore.

Charleton and Physiology. Galen (131-201) did much physiologi-

cal work, but often used the word physiology in the wide sense of natural philosophy, and this connotation held the field for many centuries. The first to confine the word to the modern meaning—the study of the normal functions of the living—was Jean Fernel (? 1497-1558) in his *Medicina* (1554) which was divided into three parts—physiology, pathology, and therapeutics; the physiological part, subdivided into seven books, included structure and function, and was on modern lines (Sherrington; Fulton). Until quite recently Fernel's pioneer work in this respect was overlooked, and the *De homine, figuris, et latinitate donatus a F. Schuyl* (1662) of René Descartes (1596-1650) has been regarded as " the first text-book of physiology after the modern fashion though in a popular way " (M. Foster). Charleton was the first to write a text-book on physiology in English in 1559 under the title *Natural history of nutrition, life, and voluntary motion. Containing all the new discoveries of anatomists and most probable opinions of physicians concerning the oeconomie of human nature*; it was dedicated to Thomas Viscount Fauconberg and Sir G. Ent, and promptly translated into Latin: *Oeconomia animalis novis in medicina hypothesibus superstructa et mechanice explicata* (1659-60), and went through at least six editions; it was extensively rewritten and brought out again in English in 1680 under the title *Enquiries into human nature in VI anatomic praelections*, delivered in the new theatre of the Royal College of Physicians in London with a dedicatory epistle to " the Right Worshipful Sir John Cutler, knight and baronet " London 1680). The six praelections dealt with nutrition, the historia ventriculi (stomach), the action and uses of the ventricle, life, fevers, and voluntary motion. The volume contained as frontispiece an engraving by Laggon of Charleton's portrait (Fig. 2).

Chorea gigantum, or the most famous antiquity of Great Britain, Stonehenge, standing on Salisbury Plain, now restored to the Danes (1668) was written by Charleton in the following circumstances: Inigo Jones (1573-1652) had been commanded in 1620 by James I to investigate the history of Stonehenge; but after Inigo Jones's death his executor John Webb found a few notes only and, urged by William Harvey and Gilbert Sheldon (1598-1677), afterwards Archbishop of Canterbury, Webb in 1655 issued *The most notable*

antiquity of Great Britain, vulgarly called Stonehenge on Salisbury Plaine, restored by Inigo Jones Esquire, Architect-Generall to the late King. This stated that Stonehenge was a Roman temple, erected if not by Agricola (37-93 A. D.), somewhat later, and was originally dedicated to the God Coelius. Charleton after consulting Olaus Wormius (1588-1654) issued his book as a counterblast which was introduced by John Dryden (1631-1700) in the fine epistle described as " the noblest poem in which English science has been celebrated by an English poet " (N. Moore). In a letter dated 16 July 1663, Hooke wrote to Robert Boyle that Charleton had given to the Royal Society " an account of Aubery (Avebury) in Wiltshire, a very strange piece of antiquity and more admirable than Stonehenge."

CHARLETON'S PUBLISHED WORKS

1. *Spiritus gorgonicus VI sua saxipara exutus, sive de causis, signis, et sanatione lithiaseos diatriba*. Lugd. Bat. 1650.
2. *Ternary of Paradoxes*, 1650. Translation of J. B. Van Helmont's *De magnetica vulnerum naturali et legitima curatione*. Paris, 1621.
3. *The incongruities, impossibilities and absurdities couched under the vulgar opinion of defluxions*, 1650. Translation of J. B. Van Helmont's *Deliramenta catarrhi*.
4. *The darkness of atheism expelled by the light of nature; a physico-theological treatise*, London, 1652, pp. 365.
5. *Physiologia Epicuro-Gassendo-Charletoniana; or a fabrik of science natural, upon the hypothesis of atoms*, founded by Epicurus, repaired by Petrus Gassendus, augmented by Walter Charleton, Dr. in medicine and physician to the late Charles, Monarch of Great Britain. London, 1654.
6. *The Ephesian and Cimmerian matrons* (the Ephesian matron by Charleton. London, 1655. Another edition, translated into Latin by Bartholomew Harris: *Matrona Ephesia, sive lusus serius de amore*. London, 1665.
7. *Epicurus's morals*. London, 1656. A second edition in 1670.
8. *The immortality of the human soul demonstrated by the light of nature* (1657) ; two dialogues between Lucretius and Athanasius (Charleton) in the garden and presence of Iso-dicastes (the Marquis of Dorchester and dedicated to him). This contained a portrait of him (vide fig. 1) which some unknown grangerizer pasted into a copy now in the library of the Royal College of Surgeons of England of Charleton's *Exercitationes de differentiis & nominibus animalium quibus accedunt mantissa anatomica et quaedam de variis fossilium generibus*. Oxford, 1677.

9. *Exercitationes physico-anatomicae de oeconomia animali*, Amsterdam, 1659. A second edition with the title: *Natural History of nutrition, life, and voluntary motion.* London, 1659. A third edition: *Oeconomia superstructa et mechanice explicata.* (*Acc. ejusdem diss. epist. de ortu animae humanae.*) London, 1659-60. A fourth edition: *Oeconomia animalis novis in medicine hypothesibus superstructa et mechanice explicata.* London, 1669. Other editions 1675 and 1680 at the Hague with the same title, and *Enquiries into human nature in VI anatomic praelections.* London, 1680.
10. *An imperfect pourtraieture of his sacred Majesty Charls II.* London, 1661.
11. *Exercitationes pathologicae in quibus morborum penè omnium natura, generatio et causae, ex novis anatomicorum inventis sedulò inquiruntur*, pp. 208. London, 1661. Dedicated to Charles II. A hypothetical discussion of the causes of disease, such as hatred as the cause of leprosy and gout; without records of cases or necropsies.
12. *Chorea gigantum, or the most famous antiquity of Great Britain, Stonehenge standing in Salisbury Plain, restored to the Danes.* London, 1663. Second edition 1725.
13. *Inquisitiones II anatomico-physicae: prior de fulmine: altera de proprietatibus cerebri humani.* London, 1665. With the imprimatur of Lord Brouncker, president of the Royal Society, to whom it was dedicated.
14. *Guglielmi Ducis Novicastrensis vita*, London, 1668. A translation into Latin of Margaret Cavendish's life of her husband.
15. *Onomasticon zoicon, plerorumque animalium differentias et nomina propria pluribus linguis exponens, Cui acc. mantissa anatom., et quaedam de variis fossilium generibus.* London, 1668. Received the imprimatur of the president and censors of the Royal College of Physicians. Second edition 1671. Third edition, Oxford, 1677. Gunther spoke of Charleton as in several respects a forerunner of Linnaeus, having emphasized the importance of nomenclature, and having a clear idea of genera and species.
16. *Two discourses: I concerning the different wits of men, written at the request of a gentleman eminent in virtue, learning, and fortune; in the year 1664.* John Locke (1632-1704) has been thought to have taken some of his ideas from this source. *II Of the mysterie of Vintners or a brief discourse concerning the various sicknesses of Wines and their respective remedies.* Delivered to the Royal Society, assembled in Gresham Colledge on 26 of November, Anno Dom. 1662. This volume appeared in 1669. Another edition entitled *Two discourses concerning the different wits of men and the various sicknesses of wines. The art and mystery of Vintners.* 3 pts. London, 1692.
17. *De scorbuto liber singularis* autoro Gualtero Charleton, M. D. Serenissimi potentissimi Caroli II Magnae Britanniae Regi Medico ordinario

et inclyti medicorum Londinensium Collegii socio (1672). Lugd. Bat. Dedicated to George Ent, Eques auratus, then president of the Royal College of Physicians in London.

18. *Natural history of the passions.* London, 1674. A translation from Senault. In the dedication to "a person of honor" it is described as "the product of my late ten weeks' solitude in the country remote from my library." Second edition 1701.
19. *Socrates triumphant, or Plato's apology for Socrates.* London, 1675.
20. *Oratio anniversaria* (Harveiana). 1680.
21. *The harmony of nature and positive divine laws.* London, 1682.
22. *Three anatomic lectures.* London, 1683.
23. *Inquisitio physicae de causis catameniorum et uteri rheumatismo* (1685). London. Approved by the president and censors of the Royal College of Physicians in London. This was reprinted with the expanded title: *Inquisitiones medico-physicae, sive flexus menstrui; nec non uteri rheumatismo sive fluore alba.* Ludg. Bat. 1686. It was described by Albrecht von Haller (1708-1777) as senilis sermocinatio. The cause of menstruation was ascribed to accumulation of altered digestive fluids in the uterine vessels until at fixed and regular periods their irritant effect caused their discharge.
24. Charleton contributed a praeloquium which appeared in two posthumous works of Sir Theodore Turquet de Mayerne (1573-1655): *Praxeos Mayernianae in morbis internis praedipue gravioribus et chronicis syntagma,* 1690 London. (Approved by the president and censors during Charleton's presidency). *Praxis medica: de cura gravidarum, tractatus de arthritide,* 1692.
25. *Oratio anniversaria* (Harveiana). London, 1705.

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Fig. 1.

Leopold Auenbrugger and Marianna von Priestenberg Auenbrugger

THE PORTRAIT OF LEOPOLD AUENBRUGGER

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The object of this note is to present to the reader the effigy of Leopold Auenbrugger as it appears in a painting with, at his side, his dear companion, Marianna von Priestenberg, who is handing her husband a cup of coffee, while he, almost as if to celebrate the recent printing of his "Inventum Novum," is showing to the public the frontispiece of his small but great book.

The date of the first edition of the "Inventum Novum" is 1761, when the author was 39 and his wife 31, ages, which in our opinion, correspond to each of them, so perhaps the hypothesis, expressed above, that the portrait was painted to commemorate the publication of the book, is not unfounded.

The author of the painting is unknown to me. That the portrait is the original of the discoverer of the percussion and that it is authentic is shown by the following proofs: the portrait came to me from a direct descendant of Auenbrugger, Mr. Ernst Lehmann of Graz, the great, great, great nephew of Leopold Auenbrugger's daughter, and it is described in detail in a biography of Auenbrugger by Prof. Clar of Graz, published in 1867. This document says that ever since the death of Lehmann's grandmother, the daughter of a daughter of Auenbrugger, the portrait was in the Castle of Katzenstein in Oberkrain, which belonged to the family. Shortly after 1865 it was restored by the academic painter Kurt von Goldenstein.

The frontispiece of the book which is shown in the portrait, except for the editor's mark which has been omitted, corresponds exactly to the first edition of the "Inventum Novum," as I have verified on a copy of that very rare edition which I own. The page opposite the frontispiece reproduces the suggestive "Monitorium ad Omnes Medicos" which in the original is in folio A 4.

The painting (80 by 72 centimeters) is certainly the work of a master hand whose ability shows off better in the portrait of the wife, full of expression, than in that of the famous Doctor, a little rigid in the sweet and complaisant smile.

Here are a few items on Auenbrugger which will suffice for reawakening in the reader the souvenir of the things which are certainly known to him.

Leopold Auenbrugger was born at Graz in Steiermark on the 19th of November 1722 unto Sebastian and Maria Teresa Goschatnig, proprietors of a Pension with an adjoining beer hall. After finishing his first studies in Graz, he attended the Medical Faculty of the University of Vienna where, under the guidance of Van Swieten, he obtained his Doctor's degree. After a few years of general practice and after serving for about four years in a Military Hospital, in 1751, he was appointed Director of the Spanish Hospital, one of the best in the city, a position he held for many years. In 1754 when 32 years old, he married Marianna von Priestenberg who was eight years younger than he, from whom he had two daughters. In 1783 Emperor Joseph conferred on him the title of Noble of Auenbrugg.

He died at the age of 87 in Vienna on the 18th of May, 1809.

The first edition of the "Inventum Novum ex percussione thoracis humani ut signo abstrusos interni pectoris morbos detegendi" is printed in Vienna with the type of John Thomas Trattner in 1761. A second Latin edition was printed in 1763; the third edition is a literal translation in French by Rozière de la Chassagne which came out in 1770 and the fourth, that of 1808, is a literal translation in French by Corvisart. In 1843 there is a third Latin edition followed by a German translation of Dr. Ungar with a preface by Skoda.

HOEFER AND CHEVREUL

(WITH AN EXCURSUS ON CREATIVE CENTENARIANS)

GEORGE SARTON
Cambridge, Massachusetts

Contents—I. Pioneers! O Pioneers!; II. Life of Ferdinand Hoefer (1811-78); III. His abundant publications on many subjects; IV. His character; V. His history of chemistry, 1842-43; VI. His other books on the history of science, 1872-74; VII. His history of mathematics, 1874; VIII. Eugène Chevreul (1786-1889) and his criticism of Hoefer's history of chemistry; IX. Chevreul, dean of the students of France; X. Manuel Garcia (1805-1906); XI. Luigi Cornaro (1475?-1566); XII. Fontenelle (1657-1757); XIII. Remarks on creative centenarians; XIV. Hoefer and Chevreul as historians of chemistry.

I. Pioneers! O Pioneers!

When we are asked to celebrate the jubilee of a fellow scholar we realize more keenly the brotherhood of all scholars working in the same field, irrespective of their time or locality, for they are all working together at the same task, helping one another, continuing and completing each other's efforts. Their fellowship is even more appreciated by contrast with the selfish and destructive efforts of unscholarly and careless writers who instead of improving our knowledge, however little, drag us backward, reintroduce errors or discredited theories, and in short unweave our fabric and mix our threads. It is especially commendable to honor the earlier members of our fellowship, the pioneers, for however crude their efforts may seem to-day, it is because of them that we are able to do a little better. On the occasion of the jubilee of Professor David Eugene Smith, we evoked the early historian of mathematics, Montucla (*Osiris* I, 519-67, 1936); the jubilee of Dr. Arnold C. Klebs gives us a welcome opportunity of evoking two other pioneers, Ferdinand Hoefer (1811-78) and Eugène Chevreul (1786-1889). The reason for dealing with both of them in the same essay will appear presently. Let us say at once that though both wrote extensively on many subjects concerning the history and philosophy of science, they did their best historical work in close succession and in the same field, the history of chemistry.

II. Life of Ferdinand Hoefer (1811-78)

Ferdinand Hoefer was born at Doeschnitz, a small village in Schwarzburg-Rudolstadt, Thuringia, in 1811.[1] His parents wanted him to become a priest, and saw to it that he received a good, if one-sided, education. His first teacher was the curate of his village; he then proceeded to the gymnasium of Rudolstadt. Much of his time was necessarily devoted to the study of Latin, Greek and Hebrew which were required in the theological schools of Germany; but he also managed to become familiar with many modern languages. He was a born linguist, if not a philologist. The intensity of his studies undermined his health, and a long journey was deemed necessary to reestablish it, an excellent remedy by the way especially in those days of slow communications, when travelling afoot with a rucksack was as natural as it was attractive. On May 26, 1830, young Hoefer left his birthplace and walked to Hamburg, where a friend joined him. They continued to Bremen where they boarded a ship which was sailing for England and the East Indies; they were shipwrecked near the East Frisian coast but were able at low tide to reach the island of Wangeroog, where they arrived penniless. The grand duke of Oldenburg who happened to be summering there came to their rescue, and gave them the means of travelling across Holland and Belgium. They reached Lille at the very time of the Revolution of July, and being abandoned by his companion, Hoefer found no way out but to volunteer as a conscript. He was ordered to join the regiment of Hohenlohe, acting then as a kind of foreign legion and quartered in Marseilles. He was thus obliged to cross the whole of France. No wonder that his shoes gave out in Orange, for he had walked more than five hundred leagues in two months. After a few months of military and polyglottic exercises in Marseilles, the regiment embarked in Toulon for Morea, to replace the French army of occupation established there since the naval battle of Navarino

[1] The main source of this biography is an article by A. de Bellecombe in the *Nouvelle biographie générale* (vol. 24, 845-54, Paris 1858), which we can trust for it was certainly revised by Hoefer himself, who was the director of that collection. Add to that Hoefer's Fragments d'un voyage en Allemagne, en Hollande, en France et en Grèce (*L'époque*, p. 483 sq., Paris 1835), not available to me. Very little is known concerning the last twenty years of Hoefer's life. A Parisian colleague might be able to fill in that gap.

(1827). After a long navigation during which Hoefer was dangerously ill with typhus, he landed in Zante and proceeded to Navarino. In his weakened condition life was very hard to him and he had finally, as he puts it, lost "hope and sleep." Happily the regiment was disbanded in March 1831, and Hoefer having learned to speak Greek resolved to walk to Constantinople across Greece and the Balkans. However, not far from Corinth he fell into the hands of klephts; he escaped but gave up his oriental dreams, returned to Navarino, whence he sailed back to France.

These two years of romantic travel were followed by a few years of drudgery as a teacher of languages in Lyon, Nantua and Saint Etienne. While he was teaching the third class in the college of Saint Etienne (1832), he attracted the attention of the philologist Burnouf,[2] who was then general inspector of schools. Burnouf recognized Hoefer's uncommon abilities and recommended him to Victor Cousin—one of the most illustrious scholars of that time, too much praised then but perhaps too much forgotten today—who gave him as an assignment, nothing less than the translation of Kant's *Critique of Pure Reason*. The appalling task was completed by Hoefer within nine months, while he was teaching at the college of Roanne. Cousin was so pleased with the result that toward the end of the school year 1834 he invited him to come to Paris. Hoefer was for some time Cousin's secretary and assisted him in the translation of *Parmenides*, *Timaeos* and *Critias*.

In the meanwhile, he had become aware of the onesidedness of his education and he decided to complete it. He followed various courses at the Museum, studied physics under Ampère at the College of France, and chemistry under Thénard at the Sorbonne. In the meanwhile, he was supporting himself by teaching in private schools and collaborating with various journals. He thus collected enough money to register as a student at the faculty of medicine (Paris, Nov. 1835). His articles dealt with philosophy, zoology, physiology, chemistry. One of them on scientific theories[3] started a discussion

[2] That was not the great Burnouf, Eugène (1801-52), one of the founders of Sanskrit and Avestic philology (*Osiris* 3, 206), but his father Jean Louis (1775-1844), a classical scholar.

[3] Published in 1836 in "Hermes. Echo du monde savant. Travaux des savants de tous les pays dans toutes les sciences."

between him and Etienne Geoffroy Saint Hilaire—the dictator Cuvier was then out of the way (he had died in 1832)—which was very creditable to both and was partly reprinted later by Achille Comte in his *Traité complet d'histoire naturelle* (1844-49, vol. 3, 97-113). Geoffrey Saint Hilaire was then sixty-four years of age and illustrious, Hoefer an unknown medical student and journalist of twenty-five, but the older man was generous; he was touched by his antagonist's idealism and honesty, came to visit him in his garret and gave him his friendship.

Hoefer obtained his doctorate in medicine on Jan. 30, 1840, with a thesis on the green sickness (chlorosis) including a historical introduction, and began to practice in a popular quarter of Paris. In the following year he published a short paper on the medical use of platinum.[4] That might have been the beginning of a promising medical career, but Hoefer was obviously bent in a different direction. He had already obtained considerable ease in the composition of scientific papers and books, and was spending most of his time in the production of them. Indeed, they followed one another in a steady stream. The French government entrusted missions to him on two occasions; the first, to study the teaching and practice of medicine in Germany (1844), the second, to study the teaching of agriculture in the same country (1847). These missions brought him in touch with many distinguished professors of his native land. Yet his main activity was writing—popular scientific writing—editing, compiling, and for more than thirty years he did little else. He died at Brunoy (Seine et Oise) in May 1778, at the age of 67. As he was not a member of the university and had been left out of the academies, and for other reasons which I shall set forth presently, his passing away hardly attracted the attention of his contemporaries. According to Fröhner, his library was sold for two thousand francs in 1882.[5] That is a small amount even for that time; it suggests that Hoefer's library was not important, or else that only a part of it was undispersed four years after his death.

[4] *Observations et recherches expérimentales sur le platine considéré comme agent physiologique et thérapeutique, ou de l'efficacité des préparations de platine dans le traitement des maladies syphilitiques, dartreuses et rhumatismales* (32 p., Paris 1841).

[5] Letter from Wilhelm Fröhner (1834-1925), Karlsruhe archaeologist, to Paul Tannery (Tannery's *Mémoires*, vol. 14, 568; *Isis* 29, 157).

III. Hoefer's Abundant Publications on Many Subjects

We are interested only in Hoefer's writings on the history of science but in order to clear the ground it is well to deal first with his other writings. This will give the reader some idea of Hoefer's enormous output and of his intellectual habits. He compiled (or took part in the compilation of) many volumes of the collection " L'univers. Histoire et description de tous les peuples," published by Firmin Didot, namely those dealing with Southern, Eastern and Central Africa, and Morocco (1848), Algeria (1850), the Near East (1852), Egypt (1877); he wrote two memoirs on the non-authenticity of the ruins of Nineveh (1850) [6] which caused much discussion.

In 1846 he compiled a dictionary of physics and chemistry which was reprinted in 1851 and 1857, and translated into Spanish (Madrid 1852). In 1847, he edited a dictionary of practical medicine. He compiled a dictionary of practical botany in 1850 (this was also translated into Spanish) and a dictionary of agriculture and horticulture in 1855. That is within ten years he produced four scientific dictionaries.

A third group of his publications might be classified under " natural history." The first and perhaps the most important of these was a new annotated edition (1856) of Cuvier's *Discours sur les révolutions du globe* which had then already attained the dignity of a classic. Hoefer also prepared a new edition of Edmond Audouit's *L'Herbier des demoiselles ou Traité complet de botanique*, in 1865 (that book had first appeared in 1847). He wrote two volumes on the seasons " Les saisons. Etudes de la nature (1867-68) " which was later adapted in English by W. H. Davenport Adams under the title " Every day objects, or Picturesque aspects of natural history " (London 1877). In 1868, another book of his of the same kind " Le monde des bois, plantes et animaux " was also published. Finally, " L'homme devant ses oeuvres " appeared in 1872, but under the pseudonym Jean L'Ermite (perhaps there were already far too many books under his own name). It was reprinted in 1890 with a preface by Camille Flammarion.

[6] All the publications named in this article were issued in Paris except when another place is named.

A fourth group is chemical. " Elements de chimie minérale, précédés d'un abrégé de l'histoire de la science et suivis d'un exposé des éléments de chimie organique (1841)," all of that in some 650 pages. This was translated into Italian (2 vols., Modena 1845). " Nomenclature et classification chimiques, suivies d'un lexique historique et synonymique, comprenant les noms anciens, les formules, les noms nouveaux, le nom de l'auteur et la date de la découverte des principaux produits de la chimie " (190 p., 1845), also translated into Italian. " Annuaire de chimie, comprenant les applications de cette science à la médecine et à la pharmacie " (1845, 1846). " La chimie enseignée par la biographie de ses fondateurs, R. Boyle, Lavoisier, Priestley, Scheele, Davy, etc." (310 p., 1865). Note that these books, except the Annuaire (which was edited by him, not written), reveal a strong historical bent.

In addition to all of these works, the writing or editing of which would have taxed the energy of a whole team, he undertook the edition of a new biographical collection, the *Nouvelle biographie générale* (or universelle), forty-six volumes published by Firmin-Didot under his direction between 1852 and 1866. On the whole a very good collection for its times; its standards are inferior to those of the national biographical collections which were to appear somewhat later,[7] yet it was a scholarly undertaking, wherein the sources of information are often quoted. Moreover, many of the articles are signed and hence the wary reader is given a means of weighing their importance. It do not know how many articles Hoefer contributed himself, and to what extent he guided and helped his collaborators or revised and corrected their contributions. The following articles were written by him: Alexander the Great, Aristotle, Roger Bacon, Caesar, Columbus, Descartes, Erasmus, Fermat, Frederick II, Herschel, Plato, Plotinos, Porphyros, Proclos, Pythagoras, Siegfried Weiss, and there are probably many others. They are scattered throughout the whole series and thus range in date from 1852 to 1866. This was a gigantic task in itself, the production of forty-six scholarly volumes containing biographies of thousands of men of all kinds, times and places,—within fifteen years, that is, an average of a little more than three volumes a year.

[7] ADB, 1871-1910, DNB, 1885-1900, DAB, 1928-37.

And that is not all, for in the meanwhile Hoefer had published translations of Aristotle's *Politics* and *Economics* (1843), of the *Library* of Diodoros of Sicily (4 vol. 1846),[8] a new translation of Berzelius' treatise on chemistry (6 vols. 1845-50), a new translation of Humboldt's *Ansichten der Natur* (2 vols. 1850-51). In 1858, he was meditating a book on the value and use of human forces, which he hoped might be his magnum opus, but it did not materialize.

Mind you this tedious enumeration (which is not complete for minor items have been omitted) was made simply to clear the ground. We have already named many more books than the average scholar could produce and yet we have not yet spoken of the books—six in number—which must be considered his main or most original works and constitute his sole defense against oblivion. However, before dealing with these we may already form an opinion of his activities.

IV. Hoefer's Character

It should be clear enough by now that Hoefer was the type of man who writes books for publishers according to their needs and offers, or to put it bluntly, he was primarily and remained throughout his life a hackwriter. He knew it and was aware of the disapprobation of the learned world as can be proved by the pathetic statement occurring at the beginning of the second edition (1866-69) of his history of chemistry, the only one of the endless series of books bearing his name which may eventually secure his admission into the paradise of scholars.

> Je respecte trop le public pour l'entretenir d'affaires personnelles. Je me bornerai donc à déclarer que les mêmes difficultés se renouvelèrent, quand je voulus reprendre mon projet. Les éditeurs, auxquels je m'adressais, refusèrent poliment de publier l'histoire de la science qui devait faire suite à celle de la chimie: l'un me commandait un lexique, un autre des traductions de grec et d'allemand, un troisième des ouvrages de géographie, d'archéologie et d'histoire, des dictionnaires de tout genre, etc., etc. Les années se passèrent ainsi, la liste de mes travaux s'allongea démesurément; mais l'oeuvre de mon choix ne devait point s'y trouver.
>
> Pourquoi, me demandera-t-on peut-être, pourquoi n'avoir pas fait ce dont vous vous sentiez capable?

[8] As to Diodoros of Sicily (I-2 B. C.), Hoefer claims that he corrected various passages relative to the history of science.

Primum vivere, deinde philosophare, répondrai-je avec un ancien.

Je ne suis rien, et je ne prétends à rien; une petite place au soleil, voilà tout ce que j'ambitionne. Mais je m'indigne jusqu'au fond de mon âme, lorsque j'entends de célèbres professeurs et académiciens reprocher à Kepler d'avoir fait, pour les libraires, de petits almanachs prophétiques, regardant ces travaux de commande comme au-dessous de la dignité du "législateur du ciel."—Eh! morbleu! fallait-il qu'il mourût de faim? Kepler s'était fait astrologue, comme un autre pouvait se faire compilateur, pour vivre.

Que d'hommes qui passent inconnus, parce que les dures conditions de la vie ne permettent point de tirer de leur esprit tous les trésors qu'il renferme!

That confession is painful to read and it awakens a deep sympathy in our hearts for the unfortunate man who felt driven to write it, but is it convincing? The question is, was Hoefer a hackwriter only because of outside circumstances, or was he partly so because of inside ones? One can see how his fate was gradually clinched. When he landed in France at the end of his adventurous wanderings, he was almost penniless and obliged to earn a living at once. His first resource was teaching but Cousin took him out of that and started him on his literary career. Hoefer soon found that the drudgery of writing was less tyrannical, left a man more freedom, than the drudgery of teaching. He was led to write more and more on almost any philosophical or scientific subject because that came easy to him. On the other hand publishers are always on the lookout for men of that very type, men who are sufficiently competent and dependable to help them carry out their programs. They know from experience that there are men who can always be tempted to write or edit a book on this or that subject for a consideration, and who will do it well, that is, well enough for their purpose, and fast, that is, fast enough to satisfy their impatient desires. It was Hoefer's misfortune not that he was poor but that he was a man of the type which has just been described. If he had been more of a genuine scholar, he would not have been so ready to compromise with the world and put himself in the hands of publishers; he would have preferred to starve a little longer or to die. And the chances are, intelligent as he was, that he would not have starved very much longer or died much earlier. Many famous scholars started their career in dire poverty and emerged from it. Hoefer began his literary life with a fine purpose but obviously he lacked strength. A man with a strong will is

like a rotating gyroscope which it is hard to deflect; Hoefer alas! was not a gyroscope but rather a weathercock which publishers could turn by blowing. It is as vain to try to apportion the blame and to determine whether Hoefer was the victim of outside or inside circumstances, as to say, who is the most guilty, the prostitute, the pander or the patron, for as such responsibilities cannot be disentangled from each other, they must needs be shared.

When a penniless scholar begins to write for money, he is in danger; if he succeeds at all—financially—he is in great danger; and if the use that can be made of him is realized by an enterprising publisher, he is lost. That is the story of Hoefer.

V. Hoefer's History of Chemistry, 1842-43

Yet Hoefer had made a good start with the publication of his *Histoire de la chimie*,* the first volume of which appeared in 1842, and the second in the following year. That was to some extent a pioneer undertaking. It is true there were many forerunners. Hoefer mentions only one, Johann Friedrich Gmelin (*Geschichte der Chemie seit dem Wiederaufleben der Wissenschaften bis an das Ende des 18. Jahrhunderts,* 3 vols., Göttingen 1797-99), observing that Gmelin omitted completely the ancient times. By the way, Hoefer's treatment of ancient chemistry was perhaps the best part, and certainly the most original, of his own effort. There were other harbingers, unknown to Hoefer or at least unnamed by him, such as the Germans, Johann Christian Wiegleb (1732-1800), Johann Bartholomäus Trommsdorff (1770-1837), Karl Christoph Schmieder (1778-1850); the Frenchman Jean Baptiste Dumas (1800-84) whose *Leçons sur la philosophie chimique* (1837) were largely historical; the Scotsman Thomas Thomson (1773-1852) who wrote a *History of chemistry* in two volumes (London 1830-31); the Englishman William Whewell (1794-1866), whose *History of the inductive sciences* contains a chemical section (vol. 3, 105-84, London 1837). Dumas' work he could not help knowing, but it is possible

* *Histoire de la chimie depuis les temps les plus reculés jusqu'à notre époque; comprenant une analyse détaillée des manuscrits alchimiques de la Bibliothèque royale de Paris; un exposé des doctrines cabalistiques sur la pierre philosophale; l'histoire de la pharmacologie, de la métallurgie, et en général des sciences et des arts qui se rattachent à la chimie,* etc. (2 vol., illus., 1046 p., 1842-43).

that he was really unaware of the English writings, for his contemporary sources were predominantly German and French.

In any case, these many anticipations of Hoefer's history were incomplete and fragmentary, and do not weaken his claim of having produced the first "complete" history of chemistry to the time of Priestley. Moreover, Hoefer had conducted a series of novel investigations, especially some concerning ancient and mediaeval alchemy, and had even examined many Greek and Latin alchemical MSS. in the Royal Library of Paris. An appendix to volume one (pp. 491-503) contains a number of original texts, one in Latin, the others in Greek, edited by him from those MSS.[94] Finally this noble effort was already conceived by him as the beginning of a greater undertaking. Said he in the preface to volume 1 (1842): "If this history of chemistry be welcomed and I receive enough encouragement in the studies to which I am planning to devote my whole life, I shall publish in succession the histories of the other physical, natural and medical sciences." That is clear. He had then at the age of thirty-one, a young man still but rich in knowledge and experience, dedicated his life to a great undertaking which might have immortalized it.

Unfortunately, his scholarly earnest was not so well received as he had expected. To begin with he had trouble in finding a publisher and the book was finally issued, in an unusual way, by Dr. Quesneville (1810-89), director of the *Revue scientifique et industrielle*, from the offices of that journal, rue Jacob 36. The first edition was sold out within eight years, and after 1850 second hand copies began to reach fancy prices; it does not follow that the financial returns to Hoefer were anything but meager; he must have realized by that time that they were out of proportion with the time consumed and the mental fatigue endured. At any rate he was discouraged and apparently abandoned for the time being his high purpose.

He allowed himself to be gradually enslaved by all kinds of mercenary projects, "Univers pittoresque," dictionaries, "herbier des demoiselles" or botany for young ladies, natural history for the general reader, chemical compilations. . . . Yet he might have been saved, for by 1866, a new edition of his history of chemistry

[94] In the second edition, a few short Greek texts were added.

was called forth and the two volumes reprinted in 1866-69 by the great publisher Firmin-Didot. In spite of its being called " revue et augmentée " the second edition does not differ materially [10] from the first, yet the twenty-five years which had elapsed since the first edition would have made a tremendous difference if the author had remained faithful to his great desire. For one thing, soon after the appearance of his first volume and before the second Hermann Kopp had begun the publication of his *Geschichte der Chemie,* which extended gradually to four volumes (Braunschweig, 1843-47). Kopp and Hoefer must have undertaken their historical investigations about the same time (1840). Since 1841, Kopp had been teaching that history at the University of Giessen. He had over Hoefer the double advantage of being a full-fledged and practical chemist, and at the same time a deeper and more persistent scholar.[11] Moreover, Kopp continued to devote much of his energy to the study of the history of chemistry and identified himself so completely with that subject, that in spite of Hoefer's initiative it is Kopp, not Hoefer, who should be called the founder of the new chemical humanism.

VI. Hoefer's Other Books on the History of Science, 1872-74

In 1845, the world might hesitate between Hoefer and Kopp, two great pioneers whose erudition in a new field was amazing. By that time, Hoefer's history was completely published, and Kopp had given three volumes of his; the two works were to some extent complementary. If Hoefer had stuck to the subject concerning which he had made such a fine beginning, he might have become the greater man, the real founder. Twenty-five years later, there could be no more hesitation. Kopp and Hoefer were no longer collaborators working together along parallel lines; the former had taken to the right and the other to the left. They would never meet again.

[10] The first edition contains 1046 p., the second 1174. The difference of 128 p. is mainly due to a new final chapter (103 p.) dealing with the development of chemistry after Priestley to Davy included. It is strangely entitled " Etat de la chimie depuis Lavoisier jusqu'à Gay-Lussac et Thénard," for it does not deal with the two last named men. The appendix at the end of vol. 1 covers 20 p. instead of 13 in the first edition.

[11] For more information on him see Max Speter: Vater Kopp (*Osiris* 5, 392-460, 1938).

Indeed, it must have been about 1780 that something terrible happened and that Hoefer's degradation was consummated. Thanks to the initiative or the complicity of the powerful publishing house of Hachette he suffered the scientific ideal of his youth to be prostituted to his evil genius. He made a contract with Hachette for the edition of a series of books on the history of science—the very project he had outlined in 1842 but in a different spirit—and within three years he published five books on the history of various sciences. One recognizes the rhythm of his earlier "literary" ventures; there must be so many books in so few years. . . . The very speed of the performance was damning him.

Needless to say these hasty compilations made by himself unaided, are not on the same level as his history of chemistry of 1842-43. They contain a wealth of information, some of it new, or at least new in that arrangement, but they are undependable. They are the kind of books which it may be sometimes expedient to consult but which should never be trusted. With regard to many special subjects his scientific preparation was as inadequate as his historical,—and the final result could not be anything but mediocre. Yet there was such a demand for those books—he was still for each of them somewhat of a pioneer—that they were often reprinted. The *Histoire de la physique et de la chimie* (561 p.) appeared in 1872, it was reprinted in 1892 and 1900; the *Histoire de la botanique, de la minéralogie et de la géologie* (412 p.) appeared also in 1872 and again in 1882; the *Histoire de la zoologie* (410 p.) in 1873 and again in 1890; the *Histoire de l'astronomie* (631 p.) in 1873; the *Histoire des mathématiques* (602 p.), first published in 1874 was reprinted without change no less than four times, in 1879, 1886, 1895, 1902. None of these books had any index or preface, except the last one including a short preface without indication of methods or sources. The mixture of elementary scientific information with history proper, typical of them all, suggests that Hoefer's conception of the history of science was still the primitive one of "introduction to science." It is of course true that an account of the genesis and early history of science is the best way of introducing the fundamental problems, but the historical method of exposition, however suggestive, is very limited. To put it briefly, it is too slow for the

scientist and too fast for the historian. Deep science and sound history are equally unpractical if not impossible on that basis.

VII. Hoefer's History of Mathematics, 1874

One of the poorest, if not the poorest, of these volumes (it is difficult to measure such things) is the history of mathematics, dedicated to the illustrious geometer Michel Chasles (1793-1880),[12] yet that very imperfect volume was printed five times in less than thirty years! Hoefer's knowledge of mathematics was so elementary and so shallow that in many cases he did not know well enough what he was talking about. Mathematicians were beginning to be hungry for the history of their knowledge, yet they had so little appreciation of historical realities and methods, and were so uncritical outside their own Elysian fields that they were satisfied with very little and utterly unable to winnow the grain from the chaff.

There were men here and there who realized the futility of Hoefer's accounts but they were very few; not only the general public but even the learned one was apparently satisfied with them. This was true not only within his life time, but even much later, at least in France. This was brought home to me by a very curious incident which I am now telling for the first time. When I began in 1912 to prepare the creation of a journal devoted to the history of science, I tried to strengthen my project by explaining it to a few of the leading men of science and learning and obtaining their sympathy and their endorsement. After having enlisted Henri Poincaré and Wilhelm Ostwald, I was able without great difficulty to constitute a very distinguished committee of patronage, the list of whose members can be read on the covers of the early parts of *Isis*. Salomon Reinach was one of the scholars whom I had invited, but he refused, one of his reasons being (believe it or not!) that the availability of Hoefer's books caused my project to be almost useless.[13] He was

[12] Himself a distinguished historian of geometry, witness the *Aperçu historique sur l'origine et le développement des méthodes en géométrie*, a very elaborate memoir published by the Belgian academy (856 p., Bruxelles 1837).

[13] I quote from memory. I have five letters addressed by Salomon Reinach to me (all except one, undated), but not that particular one. A large part of my prewar correspondence, which was filed in my Wondelgem library, was stolen by the German officers quartered in my house. At least one of those officers was interested

undoubtedly a great scholar, one of the leading classical philologists of his time, but like every one else he had his blind spots. This particular one was rather large.

Among the few experts who could appreciate Hoefer's history of mathematics and measure its hollowness were men like Moritz Cantor (1829-1920),[14] Gustaf Eneström (1852-1923) and Paul Tannery (1843-1904). The latter relieved his feelings in two letters addressed in 1884 to George Johnston Allman (1824-1904), the historian of Greek mathematics, and to Eneström. Both men had apparently written to him to elicit his opinion of the *Histoire des sciences mathématiques et physiques* of Maximilien Marie (12 vols. 1883-88) the first volume of which had just appeared. Tannery wishing to express his disapproval of that work as strongly as possible declared to his correspondents (the terms of both letters are different but the meaning is the same) that Marie's book was even worse than Hoefer's or rather it would be worse but for the fact that Marie at least was a mathematician, even if his mathematical views were a bit narrow " autrement il serait bien au-dessous du malencontreux Hoefer." [15]

This Maximilien Marie (1819-91) by the way, was a Parisian and Polytechnician, a student and later instructor and examiner in that famous school. His sister, Madame Clotilde de Vaux (1815-46), was destined to become the holy mother of Positivism; it was at Maximilien's home that Auguste Comte met her. Charles de Rouvre (b. 1871), who wrote the fantastic story of Auguste and Clotilde's love, was Maximilien's grandson.[16]

VIII. Eugène Chevreul (1786-1889) and His Criticism of Hoefer's History of Chemistry

Leaving out of consideration the *Nouvelle biographie générale* which has not yet lost its usefulness to-day, especially for territories like France not covered by the modern national biographies, Hoefer's

in the history of mathematics for he took away many of my books on that subject. The Reinach letter and other autographs may turn up some day in the trade.

[14] In his *Vorlesungen*, Cantor mentioned Montucla often, Hoefer never.

[15] P. Tannery: *Mémoires scientifiques* (vol. 13, 66; 14, 322; *Isis* 24, 162; 29, 157).

[16] *Isis* 26, 476.

most valuable book is his history of chemistry for the preparation of which he had taken considerable pains. He was a true pioneer in that field, especially with regard to ancient and mediaeval times. It is typical that even that work, his best work, was not brought up to date; the first edition told the story down to Priestley and the second extended it a little further, to Davy. Since then half a century had elapsed during which an immense amount of work had been done in many directions; for example, organic chemistry had been enormously developed. In the preface, dated October 1868, to the new volume two, Hoefer played with the idea of devoting a third volume to the nineteenth century. He even thought himself especially well qualified because he was not a professional chemist (like Chevreul and Dumas),—because of his "philosophical isolation." A strange qualification indeed. However, nothing came out of that; Hoefer wrote a series of other books, but no third part to the monument of his youth; that did not pay. It is significant also that his history of chemistry was the only one of his many histories of science for which he took the trouble to compile an index. As to the others, he would probably have said as careless compilers will say, "A quoi bon? why should we go out of our way to help critics find out our sins? . . ."

Hoefer's history of chemistry was decidedly a novelty, especially in the French language, and it attracted the attention of one of the leading chemists of that time, one of the most interesting scientific personalities of the last century, Eugène Chevreul (1786-1889). The latter's services are not sufficiently appreciated today, for the development of organic chemistry has been so exuberant since the best years of Chevreul (1813-24) that it requires not only knowledge but imagination to see the pioneers in their own background and their true size. As a matter of fact, Chevreul's *Recherches sur les corps gras d'origine animale* (1823) and his *Considérations générales sur l'analyse organique et ses applications* (1824) are landmarks in the history of chemistry. Their fundamental value has been clearly indicated by a master of the following generation, Marcelin Berthelot.[17]

[17] I quote the concluding paragraph of Berthelot's eulogy of Chevreul, read to the academy in 1902 (*Mémoires de l'Académie des sciences*, vol. 47, 1904; 48 p.)

Aux notions imparfaites et flottantes régnantes à son époque et qui tendaient à confondre sous des appellations vagues tout un ensemble de composés analogues, il a substitué des idées claires, précises, fondées sur une logique rigoureuse. Il a imposé aux chimistes et aux physiologistes une discipline inflexible, jusque-là inconnue dans les études de chimie végétale et animale. Les services de Chevreul à cet égard sont comparables, sous certains rapports, à ceux que les logiciens scolastiques ont rendus à la raison humaine. C'est surtout par ce côté de ses travaux que Chevreul s'est rattaché à la tradition philosophique des savants du XVIII[e] siècle et qu'il a concouru, pour une part inoubliable et avec une pleine conscience de son oeuvre, aux progrès généraux de la science et de l'esprit humain.

Chevreul was a pioneer not only in organic chemistry but also in our own field. His technical researches, engrossing as they were, were not sufficient to appease his curiosity. Without being either a genuine historian or a genuine philosopher, he was all the time asking himself questions which could not be answered experimentally in the laboratory, but only by patient efforts in the library and long meditations. In that respect, he was a man of his own generation, the romantic age, the age of Naturphilosophie, and his philosophical efforts suggest comparisons with those of his countrymen Ampère and Comte. He was perhaps more of a philosopher than of a historian, just like Comte and like many scientists of our own time who are aware of a philosophical need of history, because they want to know the origin and evolution of their knowledge, yet fail to realize the need of historical precision. In short, Chevreul was not satisfied with the mastery of a new technique and the discovery of new recipes; he was anxious to integrate and to understand more deeply his own knowledge; yet he who had invented new methods of organic analysis did not yet conceive the equal necessity of historical methods. How could one blame him, when the majority of the scientists of our time, including some of the leading ones, are still, after more than a century, on the same intellectual level?

Chevreul's historical curiosity antedated Hoefer's book; for example, as early as 1833 he published an article on the dowsing rod,

which is our main source for the biography of Chevreul in addition to the more recent work of Georges Bouchard (1932; *Isis* 31). For impartial appreciations of Chevreul's chemical achievements, see, e. g., Edv. Hjelt: *Geschichte der organischen Chemie* (Braunschweig 1916).

which was later developed into a book.[18] This was not simply a historical study, but also an analysis of scientific method, its limitations and applications. The logical aspect of these investigations led him later still to the writing of another book [19] on scientific method.

Hoefer's book came at the right time for Chevreul and gave a strong stimulus to the latter's historical curiosity, which was not restricted to modern times or to scientific chemistry, but extended to all ages and to alchemy and occultism. Soon after the publication of Hoefer's first volume Chevreul began to issue in the *Journal des savants* [20] a long series of articles devoted to the analysis, criticism, and discussion of it. As far as I know, no work has ever been criticized as lengthily, for Chevreul's review of it extends to fourteen articles, published within nine years (1843-51) and covers some 191 quarto pages! It is amusing to read toward the end of it the author's regret not to be able to discuss adequately the works of Bergmann, Scheele and Priestley because of the need of brevity.

Two extracts taken respectively from the first article (p. 66, 1843) and from the last (p. 229, 1851) will indicate Chevreul's purpose in writing them and help us to see more clearly that the history of chemistry was then still in an inchoate stage.

L'importance que peut avoir l'histoire d'une science n'ayant pas été toujours convenablement appréciée, nous avons saisi avec empressement l'occasion d'examiner une histoire de la chimie par le docteur Ferd. Hoefer, dont le premier volume a paru récemment. Si les considérations auxquelles nous allons nous livrer, avant de rendre compte de cet ouvrage, ne font pas partager au lecteur notre manière de voir peut-être provoqueront-elles des discussions, ou, du moins, feront-elles naître des réflexions propres à fixer l'opinion sur l'utilité dont peuvent être les histoires des sciences, lorsque les matériaux qui les composent ont été subordonnés à une méthode rationnelle, d'après laquelle les auteurs, avant de prétendre captiver par la forme et par l'agrément et la variété des details, se sont principalement proposé de faire connaitre l'esprit des sciences et la succession des idées qu'elles ont propagées, aux différentes époques de leur développement.

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[18] *De la baguette divinatoire, du pendule dit explorateur et des tables tournantes au point de vue de l'histoire, de la critique et de la méthode expérimentale* (XVI-258, 1854).

[19] *De la méthode a posteriori expérimentale et de la généralité de ses applications* (405 p., 1870).

[20] He collaborated with that Journal for fifty-seven years, 1821 to 1877.

Sous ce point de vue, la chimie se présente, à celui qui veut en tracer l'histoire, comme une science tout à fait à part, soit qu'il considére la vaste étendue du champ qu'elle embrasse et les diverses classes de connaissances dont l'ensemble en constitue le fond principal, soit qu'il cherche à y rattacher des notions qui sembleraient, à une observation peu attentive, en être absolument indépendantes, parce qu'elles émanent de sources réputées généralement étrangères à son essence. . . .

. . . Parvenu à la fin de notre tâche, nous résumerons ainsi notre jugement sur l'histoire de la chimie du docteur Hoefer.

Si nous avons combattu quelques-unes de ses opinions, désiré plus de méthode dans l'exposé des faits, nous ne voudrions pas que nos lecteurs donnassent à notre critique plus de gravité qu'elle n'en a réellement. L'instruction du docteur Hoefer est incontestable, sa connaissance des langues anciennes et modernes lui a permis de remonter aux sources originales des écrits qu'il a consultés; toutes les citations qu'il en a faites, que nous avons eu l'occasion d'examiner, au mérite du choix réunissent celui de la fidélité. Ses jugements sur les personnes, généralement équitables, sont toujours convenablement exprimés; enfin, les connaissances de l'auteur dans les sciences et dans les langues nous font regretter qu'il ne soit pas placé dans une grande bibliothèque, où il rendrait d'incontestables services à tous ceux qui s'occupent de l'histoire des sciences physiques.

Si, dans l'examen que nous venons de faire de l'ouvrage du docteur Hoefer, nous avons donné, beaucoup de développements à plusieurs points de l'histoire des faits ou des doctrines, c'est, nous l'avouerons, avec l'intention de pressentir le jugement du public sur la manière dont nous envisageons l'histoire en général et celle de la chimie en particulier. Nous serions trop heureux que nos opinions devinssent elles-mêmes un objet de discussion afin de profiter du débat qu'elles provoqueraient avant de publier nos Considerations générales sur l'Histoire de la chimie.

Much in these essays is in the nature of philosophical rather than historical discussion, and Chevreul never succeeded in emancipating himself from the tendency to premature and frivolous generalizations. Again few historians of science among our own contemporaries, and fewer philosophers, have a right to throw the first stone. In 1866, he published the first (and as it turned out the last) volume of his history of chemical knowledge,[21] which was meant to include four volumes. The plan was a little too ambitious and miscarried

[21] *Histoire des connaissances chimiques. Vol. I. Introduction. Connexions des sciences du domaine de la philosophie naturelle exposées conformément à la méthode a posteriori expérimentale sous le double rapport de l'analyse et de la synthèse* (484 p., 1866). The title illustrates Chevreul's heavy and awkward style.

because of that. This first volume contains nothing but prolegomena, on the purpose and field of chemistry, on its relationship with other sciences, with natural history and with life itself, on the classification of the sciences, on scientific abstraction versus art and literature. At that time, the author was eighty years of age and was already regarded as the dean of European chemists. He was a living link between some of the participants in the chemical revolution—he studied under Vauquelin (1763-1829) and knew Fourcroy (1755-1809)—and the founders of contemporary chemistry. For example, it was he who presented in 1860 to the academy of sciences the *Chimie organique fondée sur la synthèse* by the rising star of that year, Marcelin Berthelot.

IX. Chevreul, Dean of the Students of France

It is impossible to deal with Chevreul even in the briefest manner without speaking of his longevity which was unique in the annals of science. It is not simply the rare longevity which thrills us, but rather the linkage to which I have just referred. An exceptionally long lived man helps us to bridge the past; if he be our contemporary, we are brought vicariously a little nigher to the events which he witnessed in his youth. It has been remarked above that Chevreul collaborated for fifty-seven years with the *Journal des savants*. Many similar statements can be made about him which are equally startling. He was for sixty-one years in the service of the Gobelins, the national manufacture of tapestries, where he was called as a chemist and an expert in colors.[22] He was a member of the academy of sciences for sixty-three years. He was connected with the Muséum (ci-devant Jardin des plantes), as student, instructor and professor, for ninety years! As compared with these astoundings figures, his forty-four years of marriage seem but a short period, which many younger men have topped, but these years were very happy ones.[23]

[22] Next to organic chemistry, Chevreul's main scientific field was the study of colors, their definitions, interreactions. He published many papers and books on the subject, physical rather than chemical.

[23] The *Histoire des connaissances chimiques* of 1866 was dedicated "A la mémoire de Mme. Sophie Chevreul, née Davalet. Reconnaissance de 44 années de bonheur." They had been married in 1818; she died in 1862.

The main condition of longevity is an adequate heredity; in order to live long one must be born that way. Chevreul's parents were both longlived; his father died at 91, his mother at 93, himself at 103. On the jacket of Bouchard's recent biography of him it is stated that Chevreul witnessed in his own country (which he never left even for a day) four kings, two emperors, three republics and four revolutions. One might have written five revolutions counting as one of them the " chemical revolution " which was perhaps the most pregnant of all.

The most remarkable fact in Chevreul's case, however, is not his longevity but the continuation of his work to the very end. When he was about eighty-five years of age, instead of adding to his name on the title pages of his publications some of his professional and academic titles (according to the fashion of that time), he wrote simply " M. Chevreul, doyen des étudiants de France." This was the beginning of the Chevreul legend, but no one suspected then that he would be doing that for seventeen years. It is as if the " dean of the students " was beginning a new lease of life. He had been born at Angers on August 30, 1786; on August 30-31, 1886, his centenary was celebrated in a series of scientific gatherings in which he took an active share. On that occasion, a ' complete ' bibliography of his writings was published,[24] but it is not complete for Chevreul published a few more papers after his centenary. By that time, it had already been found necessary to limit the size of communications in the *Comptes rendus*, but Chevreul had always been prolix and more space was granted to him. A member having complained of this and asked what conditions were needed to obtain such privilege, the perpetual secretary answered " It suffices to be a hundred years old! "

Chevreul continued to attend scientific meetings until the spring of 1888, and in that year he was reelected president of the Société d'agriculture.[25] His last communication to the academy of sciences, on the rôle of nitrogen in vegetal economy, was presented on May 22, 1888, in the second year of his second hundred. After that he began to decline and died on April 9, 1889.

[24] Godefroy Malloizel: *Oeuvres scientifiques de M. E. Chevreul* (298 p., 1886).

[25] He had been admitted in 1832 and from 1849 on he was elected president and vice-president on alternate years. That is, if I count well, he was elected twenty times president and twenty times vice-president. Another record!

Chevreul was not only the dean of the French students, he was the dean of students all over the world, and he is to this day the undefeated champion of scientific old age. No other scientist has ever presented a scientific paper to an academy in his hundredth and second year. The only other scientist who comes close to him is the Spaniard, Manuel Garcia. And after Garcia I can think only of two others, Cornaro and Fontenelle, though neither of them was a real centenarian. In other words, centenarians are exceedingly few in number (about one in a hundred thousand according to Dr. R. Pearl), and these few are almost every one of them so mediocre that they suggest the survival of the unfittest.

X. Manuel Garcia (1805-1906)

Let us describe rapidly these three outstanding cases. Manuel Garcia (1805-1906)[26] was born in Zafra, Catalonia, 1805 of a very talented family of actors and musicians. He became one of the greatest singing teachers ever known, obtaining after many vicissitudes international fame in Paris, later in London. His methods have been gratefully celebrated by many students, three of whom are immortal, Jenny Lind, " the Swedish nightingale," and his own sisters Maria Malibran and Pauline Viardot. Garcia was a scientifically minded man and his professional needs led him to an important invention, to wit, the modern laryngoscope (*Proc. R. S.*, VII, 399-410, 1855). In 1906, his centenary was celebrated in London by musicians and physicians representing the main academies and conservatories of the world and like Chevreul he was able to take an active part in those impressive but exhausting ceremonies, which would have taxed to the limit the energy of younger men.

His physical appearance at the age of a hundred is perpetuated by an admirable portrait of John Singer Sargent now kept in the Providence Museum, even as Chevreul's by Roty's medal.[27] These

[26] The main sources for Garcia are the biography by one of his latest pupils, M. Sterling MacKinlay: *Garcia the centenarian and his times* (350 p., ill., New York, 1908) and the notice by Frederick Corner (DNB, 2d. supp., 2, 74-75, 1912). I owe the communication of Mackinlay's book and other documents to the Boston Medical Library and its director, Dr. Henry R. Viets.

[27] The portrait was bought by that museum in 1919. A photograph and description of it may be found in the *Bulletin of the Rhode Island School of Design* (vol. 7,

two monuments, Sargent's portrait and Roty's medal are the most beautiful iconographic documents concerning " homo centenarius." The two men evoked by them look much younger than their age and still full of intelligence. These portraits form a great contrast with the portraits of centenarians appearing occasionally in the newspapers, most of which are rather painful to look at.

For Garcia as well as for Chevreul the centennial festivities did not put a complete stop to his work. He continued to advise a few selected disciples and to attend concerts; he corrected proofs in his own hand in his hundredth and second year, and died smoothly in London in 1906.

Garcia's family was not as long lived as Chevreul's. His father died at 57, his mother at 83 or 84. The Malibran died very young (28), though in full glory in 1836, her death being due to a riding accident; Madame Viardot-Garcia died only in 1910 at the age of 89.

XI. Luigi Cornaro (1475?-1566)

We need not give so much space to the two others—Cornaro and Fontenelle—for they are taken in only by special courtesy. Neither actually reached his hundredth anniversary.

The main interest of Cornaro's case lies in the fact that he was not only a centenarian, or at least a nonagenarian, but also a theoretician of macrobiotics. Luigi (or Alvise) Cornaro was born in Venice,[28] being a scion of the illustrious Corner family. He reclaimed the marshy land of Codevigo near Padova, carried out many agricultural improvements, erected a number of farm houses and other buildings, and by his knowledge and resourcefulness increased considerably the value of his estates. Unfortunately, his material success led him to indulge in dissipation. At the age of about thirty-five his health was so undermined by his intemperate life that he suffered all kinds of ailments and hoped to die. Then of a sudden he pulled himself together and completely changed his manner of life. He became extremely temperate (if these two words be allowed to stand together), reducing his daily diet to twelve ounces of solid food

37-39, 1919). Thanks to Mr. Alexander Dorner, director of that very beautiful museum, for his courtesy. For the Roty medal of Chevreul, see *Isis* 8, 333.

[28] The date is uncertain. The various dates quoted vary from 1464 to 1475.

and fourteen ounces of wine, and reducing it still further as he grew older. The change improved his health considerably, made a new man of him, and he gained more and more faith in his regimen. As he grew older he was more and more proud of his way of life; in fact he became a kind of faddist and decided to publish his experience for the benefit of his fellowmen. He was already an old man when he wrote his *Trattato della vita sobria*, and having done so he did not lay his pen down but from time to time wrote three additional discorsi on the same subject. The book was published in Padua 1558 when the author was at least 83 years old and perhaps 91;[29] in any case he was already old enough to give himself as an example of the validity of his theory. He remained in good health to the end of his life and died in Padua on May 8, 1566. It is typical of him that he had requested the executors of his will to make an autopsy of his body and to witness the integrity of his organs. I do not know whether the autopsy took place; no document relative to it has come down to us.

There is a beautiful portrait of him in the Pitti gallery, Florence, formerly ascribed to Tiziano, now to Tintoretto. It represents Cornaro as an old man, with a white beard and a bald head; he might be almost any age above seventy; unfortunately the portrait is undated.

XII. Fontenelle (1657-1757)

When Fontenelle's birth and death years are quoted, 1657-1757, one may think that he was a centenarian, but as a matter of fact he fell short of the goal by thirty-two days. He was a nephew of the great Corneille and like him born in Rouen, though half a century later. He was very frail in his youth and thus obliged to take good care of himself. This became an ingrained habit and we might say the main occupation of his life, next to his intellectual work. He

[29] According to Senator Davide Giordano in his interesting article "Della fame, che hanno gli uomini, di anni" (*R. Istituto veneto di scienze*, 27 p., Venezia 1932), Cornaro's four discourses were written respectively at the ages of 83, 86, 91, 95. This was based on the assumption that he was born in 1467. In a kind letter addressed to me on Sept. 2, 1939, he tells me that the new dates established by Professor Vittorio Rossi are now considered the most probable: that is, birth in 1475; death on May 8, 1566. These are the dates which I have accepted. On that basis Cornaro's discourses were composed at the ages of 75, 78, 83, 87. Very few people write enduring books at the age of 75, not to speak of 87.

always did everything he could, and avoided everything he should, to insure his ease and peace. He was a theoretician not like Cornaro, of longevity, but of happiness which comes close to it. He worked indefatigably almost until the end of his life, though he became deaf in old age and during the last six years was almost blind. He had been elected perpetual secretary of the academy of sciences in 1697, and in that capacity he wrote a number of eulogies of his colleagues which remain to this day his best title to fame.

XIII. Remarks on Creative Centenarians

When some one of our neighbors passes the mystical barrier of his hundredth birthday, he receives much publicity. The centenarian and his family are duly interviewed. The main and obvious question is always, "How did you do it?" Our readers may remember the great variety of answers. For example, one old man states that he never drank or smoked; while another admits that he always drank and never abandoned his faithful pipe. No valuable information can be derived from such conflicting testimonies. According to Raymond Pearl who has investigated a great many of those reports, the fundamental cause of longevity is an adequate heredity. Another condition recognized by him, as being very frequent if not universal, is placidity. That is, while other conditions as to diet and habit vary considerably from man to man, almost every centenarian was characterized by a high degree of placidity. It is not clear to me, from Pearl's account, whether that placidity was a cause of old age or a result of it. Had the placid centenarians, whose records he scrutinized, been equally placid in their youth?

In any case, we find that qualification, sufficient placidity, remarkably well developed in the four scientific centenarians—Cornaro, Fontenelle, Chevreul and Garcia.

For the first, placidity and her sister temperance (it would be difficult to reconcile placidity with intemperance) were the very basis of his macrobiotic method. Fontenelle always tried to reduce his worries and to live as softly and quietly as was possible. Apparently Chevreul and Garcia did the same. They had both been indelibly impressed in their childhood by the cruelties of wars and revolutions. Both had witnessed the wanton killing and torturing of

innocent people. Both drew from the awful experiences to which they had been submitted in their most impressionable years a profound horror not only of wars and revolutions but also of what they called politics; instinctively both organized their activities as far away from the madding crowd as they knew. In addition, both were very temperate and methodical, and thus the tenor of their days ran as evenly as it was achievable.

Berthelot met Chevreul for the first time about 1849 when the latter was 63. He described him then as " a tall old man, whose head is crowned with long hair moved by the wind, robust and debonair." That description fits well with the Roty medal sculptured thirty-seven years later. As a matter of fact, Berthelot, who saw Chevreul frequently (they were fellow academicians for sixteen years), remarks that the latter remained very much alike for forty years. This is plausible and the more so that, while Chevreul grew from 63 to 103, Berthelot developed naturally much faster from 22 to 62. To the young man of 22 Chevreul was already an old man when he first met him, and continued an old man, while the youth was gradually traversing maturity and reaching old age himself.

It is characteristic of Chevreul's " placidity " that from 1870 to his death in 1889, that is, during his years as " dean of the students of France " he never opened a newspaper and restricted his reading to scientific and literary writings. The greatest shocks of his adult life had been the siege of Paris in 1870, the bombardment of his beloved Muséum by the Prussians, and the Commune. He remained in Paris as was his duty. Garcia, a true déraciné (though he continued as a Spanish subject until his death) left Paris for London when the unrest grew in the French capital. That was also his duty as well as his privilege, or more simply the instinctive defense of his profound need of peace.

Pearl's conclusion is thus amply verified in these four cases. People who are born long-lived, increase their chances of longevity in proportion to their placidity and temperance. Placidity by the way is the general, neutral, term which applies equally well to every centenarian, whether he be of the usual, unthinking, animal type, or of the very unusual intelligent and creative one. The first are placid

like cows and sheep; their success in long living seems to be due in part to their lack of sensibility and imagination, to their spiritual inertness, to their very mediocrity. The latter become placid, if they can, once they have fully realized the uselessness of bothering overmuch about things which are beyond their control. Placidity is then transformed into something higher, equanimity. We may conclude that Cornaro, Fontenelle, Chevreul and Garcia owed their abnormally long span of life, of intelligent and creative life, at least in part to their equanimity.[30]

XIV. Hoefer and Chevreul, as Historians of Chemistry

To return to the two Frenchmen, Hoefer and Chevreul,[31] both were pioneers in the field of the history and philosophy of science. Their best historical work concerns the history of chemistry. Hoefer's history and Chevreul's commentary still deserve to be consulted by historians of chemistry today, but with great care. To put it otherwise these two complementary works may still be of service to critical and wary historians; they should not be left in the hands of an inexperienced student or of that elusive and reckless gentleman "the general reader." None of Hoefer's books should be placed on the open shelves of reference libraries, as is still the case in too many of them, especially in France.

Hoefer was a better historian than Chevreul, for that was after all his profession, but Chevreul was a better philosopher. The latter surpasses the former as the man who knows something deeply—in this case, organic chemistry—is always bound to excel him who knows nothing at first hand or who knows nothing thoroughly. Chevreul worked in laboratories almost to the end of his life and his faithful attendance at the Académie and at the Société d'agriculture, kept him in close touch with other workers like himself. Scien-

[30] As the word equanimity has a noble connotation, I must add lest the reader be misguided, that equanimity as well as placidity, often implies a good amount of selfishness, of coldness, and even of callousness. It is so much easier to bear misfortunes, especially of other people, if one does not take them to heart.

[31] Chevreul was a Norman; Hoefer, a Thuringian yet a Frenchman by adoption. He was naturalized in 1848, having been made a knight of the legion of honor two years earlier.

tific realities were always concrete and alive in his mind. Partly to be sure because of his old age, but also because of the original work of his earlier days, he enjoyed considerable prestige in scientific circles and was looked up to with increasing veneration not only by the younger fellows, but also by such illustrious men as Claude Bernard, Dumas, Pasteur and Berthelot. It is significant that Bernard quoted Chevreul's views many times in his philosophical notes recently edited by Jacques Chevalier (Paris n. d. 1937?).

Hoefer and Chevreul deserve the gratitude of historians of science and especially of historians of chemistry. They were among the first chemists to realize that in spite of the fantastic progress of that branch of science or maybe because of it, it is never enough to know the crude facts and latest theories, one must know their origin and their evolution. The chemist who does not care for history may laugh at Hoefer and Chevreul and even at their better trained successors, but by so doing he reveals his own lack of humanity and of imagination, which no amount of cunning and efficiency can compensate or justify.

A NOTE ON THE FRITZSCHE-KLEBS DESCRIPTION OF ACROMEGALY (1884)

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In introducing a new series of articles on medical eponyms, *The New England Medical Journal* (1) observes that "the most graphic and essentially the best description of a clinical syndrome is commonly that given by its discoverer in the first flush of his enthusiasm." In venturing now to direct attention to a full account of acromegaly which preceded by two years the often cited description of Pierre Marie (1886) I do so without apology because this excellent report is so little known * and because it was written with that lucidity which is likely to come with the flush of original enthusiasm. And it is, moreover, appropriate that it should be mentioned here since one of the authors was the father of Arnold Klebs (3).

The description was published by E. Fritzsche ** and Edwin Klebs as a separate monograph of 89 pages which appeared in 1884 from the press of F. C. W. Vogel of Leipzig under the title, *Ein Beitrag zur Pathologie des Riesenwuchses* (7). Pierre Marie(12) did not refer to the Fritzsche-Klebs case in his report in 1886 and Minkowski (18) in 1887 expressed regret that this report was so little known. The apparent rarity of the book will justify the quotations which are made from it.

There had been several accounts of authentic cases of acromegaly before this. The disease itself dates back assuredly to the 16th century for historical evidence of it is found in portraits and sculpture of that century. Adequately described case reports appeared in the medical literature by the 18th century. Patry's thesis, (20) dedicated to Pierre Marie, will be found invaluable for it contains most

* Until 1931 it had even escaped the attention of the late Harvey Cushing who had devoted so much time to the study of acromegaly.

** The name appears as "Fritsche" on the monograph but is correctly spelled "Fritzsche."

of the earlier case reports, either in original or translation. Of these, those of Noel, (19) Saucerotte, (21) Brigidi, (4) Lombroso, (9) and Verga (23) are of greatest interest. Noel writing in 1779 and Saucerotte in 1801, both describing the same individual, reported what appears to be the first authentic case of acromegaly. In 1839, Magendie, (10) in his lectures on the physiology of the nervous system included brief notes on two women patients, probably acromegalics. Verga (1868) was the first to record a complete post-mortem on a recognized case of the disease. He found a large tumor which had filled an enlarged sella and burrowed its way into the sphenoid bone. Brigidi's case (1877) was a 65 year old actor, Ghirlinzoni, with a remarkable clinical story and at autopsy, a 14-gram pituitary. Brigidi was the first to describe microscopic sections of the gland. Reference to Atkinson, (2) Sternberg, (22) Brooks, (8) or Patry (20) will give further descriptions of these cases.

The Fritzsche—Klebs Case

The clinical features of the disease, based on one case presented by Fritzsche, are not dissimilar to that of Pierre Marie's. The patient, a tall, well developed, Swiss peasant, Peter Rhyner, enjoyed good health until the age of 36 when he first noticed pains in his hands during milking. Occipital headaches, progressive loss of strength, and marked increase in the size of the hands, feet, ears, lips, and lower jaw associated with increasing kyphosis, characterized the disease during the next six years of his life. After that, the growth of the extremities became less evident. He also suffered from loss of vision, anorexia, and decrease in sexual power. First seen at the age of 44, the proportionate increase in size of hands, feet, head, jaw and kyphosis were particularly stressed by Fritzsche; atrophy of the scrotum and testes, a large tongue, brachycardia, anaemia, and bronchitis were also noted (See Fig. 1). The patient's weakness increased, he became subject to fainting attacks and finally died during one of these at the age of 44 or 45.

Klebs' discussion of this case begins with a discourse on gigantism in general, raising several questions on the influence of inheritance and environment, but clearly recognizing that the disease differed from gigantism *per se*. He considered that both conditions were

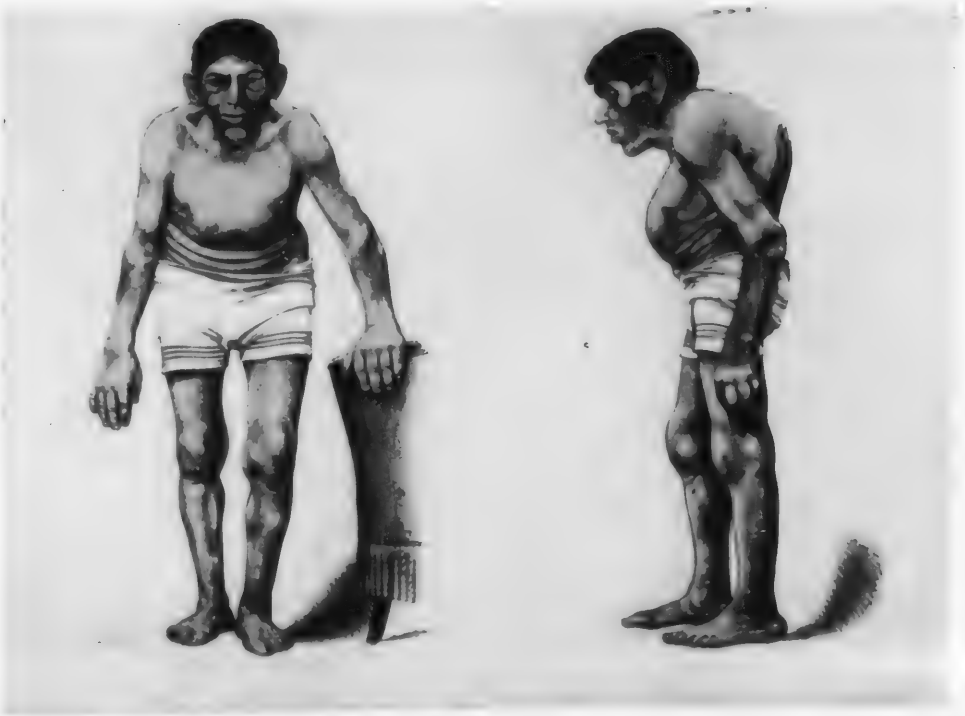

Fig. 1.

The Fritzsche-Klebs case of acromegaly

attributable to derangements of the normal growth mechanisms, but that acromegaly was distinctly a pathological process occurring late in life.

The post-mortem findings are painstakingly recorded with not

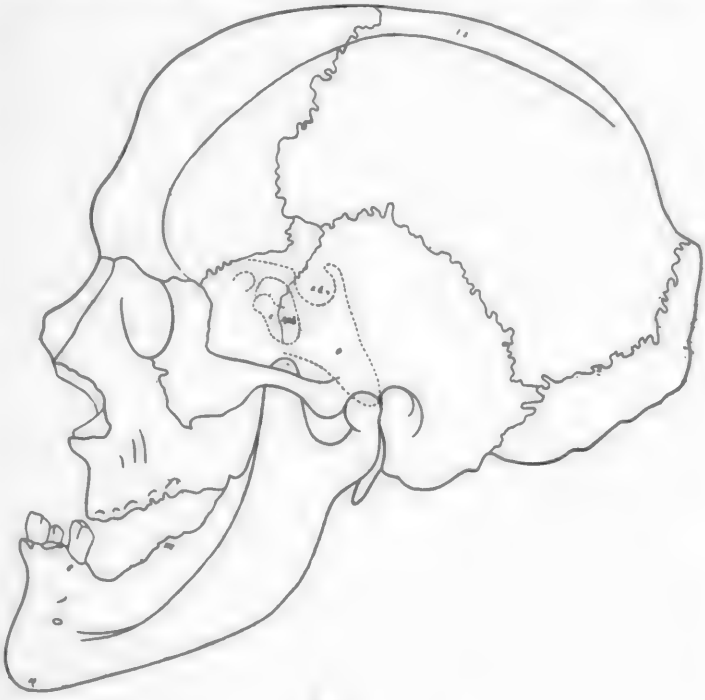

Fig. 2.

Drawing of skull from the Fritzsche-Klebs case of acromegaly.

only actual topographical and osseous measurements but comparisons were made with normal measurements (See Fig. 2).

The hypertrophy of the internal viscera was noted in detail with special emphasis on that of the thymus and hypophysis. The former was found to be normal in structure but its size (13 x 11 x 9 cm.) was enough to make Klebs go back to his autopsy records and construct from the data he found there a table in which he attempted to

prove that the largest thymus was associated with the largest child. The apparent increase in the size of the heart and the blood vessels in the case of Peter Rhyner induced him to make a most detailed study of the circulatory apparatus. He concluded that the arterial system took a far less active part than the venous in the pathological growth processes involved and, developing a rather complicated nomenclature, he explained the process as a generalized "angiomatose." And although, as we shall see, Klebs considered in some detail the question of hypophysial involvement, the condition of the thymus and the "angiostome" overshadowed in his mind the importance of the hypertrophy he found in the pituitary. Before considering the pathology of these glands, which constitutes the most interesting part of the Klebs' report, attention should be called to the careful examinations and measurements to which Fritzsche and Klebs subjected all the component parts of their patient. Such a report is as full of value today as it was then.

The Thymus

Klebs was particularly interested in the thymus and the following excerpts include all the important statements which he made concerning the histology of this gland.

"The architecture of the thymus of P. Rhyner corresponds in all essentials to that of a normal gland. I am unable to say whether the individual acini with their many interconnections have increased in number or are enlarged; from the external appearance it seems that both processes have occurred as the lobules appear in large numbers. Microscopically these are put together as in a normal gland; in the peripheral layers the spaces of the reticulum are tightly stuffed with larger and smaller round cells, while in the center—even in the celloidin preparations from which nothing can have fallen out—there is a system of wide loops, some round and some oblong, which enclose but a few cells besides the well-known thymic corpuscles. Some of these, obviously the older forms, are calcified; while others are composed of finely granular protoplasmic cells with large nucleii. These, I was surprised to find, did not show the colloidal metamorphoses accurately described by Ammann; only rarely can one find these nucleii moderately swollen like a hydropic vesicle. The cells substance is never displaced by this swelling and reduced to a small ring as sometimes happens in normal glands of young individuals. I consider these forms, therefore, as juvenile ones and have endeavored farther to investigate their development.

"It should, however, be noted that these cells range themselves with surprising frequency in longitudinal rows which are connected by narrow bands of similar flat cells, or may be separated from each other. In my preparations I have with others failed to find open vascular tracts connected with these formations as observed by Afanasiew. . . . Still I consider that a vascular connection is possible although I was not able to recognize the connecting tracts as such.

"While searching for the earlier stages of such formations, which cannot have sprung up by themselves suddenly, I became convinced that they originate in the blood vessels by a gradual massing of endothelial elements, part of which might have originated there. I noticed not infrequently in the center of the acini, cavities of a round or oval shape. These were lined with a layer of markedly projecting cells with large nucleii, which are not always in close connection with each other. Some are larger than others, projecting into the lumen, the smaller of the cavities being filled by two or three large cells, which, for the most part, are attached to the walls. Finally, they detach themselves and fill the cavity as free cells. Their further enlargement and multiplication, possibly from the outside, leads to the flattening of the cells and the formation of the thymic corpuscles. . . .

"In this way one can explain plausibly the endothelial origin of these structures, but the nature of the cavities in which they are formed is still to be explained. They lie in the center of a tissue whose lymphatic nature is not to be doubted, but this is no proof that they likewise originated in the lymphatic system. . . .

"Direct proof of the problem is unfortunately not possible since the preparation is entirely bloodless. Still I am forced to state that I believe in the vascular nature of these cavities although its demonstration must be made on other more suitable preparations.

"I am persuaded to accept this theory by the fact that these cavities with their cellular contents are frequently in connection with channels, which, though of a somewhat narrower lumen, are evenly and uniformly lined by flat endothelial cells. These are obviously portions of the capillary net-work which penetrates the follicles; in our case this is of enormous thickness, some parts being more enlarged than others. . . .

"Hence, I am of the opinion that the vascular nets of thymus follicle give rise to the vascular endothelium. These cells become detached, enter the blood stream, and are carried away, or they accumulate wherever they are formed in large numbers or where a widening of the channel favors stagnation, transforming themselves into these endothelial pearls (thymic corpuscles).

"Therefore, I, too, believe that the production of these formations is also a sign of great cellular activity in the thymus, both in young animals (Ammann and Roth) and in such animals in which the gland remains unchanged throughout life . . . and finally also in our case of abnormally growing thymus. . . .

"The necessary conclusion would seem to be that these elements have a peculiar significance in the development of the vascular system, and that one may call them *angioblasts*.

"Even though the material presented is inadequate, the established relation of the thymus in the formation of the angioblasts as well as the involution of the gland with the completion of growth may give impetus to the farther investigation of these relationships." *

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* "Der Bau der Thymus des P. Rhyner entspricht in allem Wesentlichen einer normalen Drüse. Ich vermag nicht zu sagen, ob die einzelnen, bekanntlich vielfach unter einander zusammenhängenden Acini eine Vermehrung oder Vergrösserung erfahren haben; dem äusseren Ansehen nach ist beides der Fall, indem die Läppchen zahlreich erscheinen. Mikroskopisch sind sie zusammengesetzt wie diejenigen einer normalen Drüse, in den peripheren Schichten sind die Höhlräume des Reticulum von grössern und kleinern Rundzellen dicht durchsetzt, im Centrum dagegen tritt, auch an Celloidinpräparaten, an denen Nichts herausgefallen sein kann, ein System weiter, bald rundlicher, bald länglicher Maschen zu Tage, von denen nur wenige Zellen, daneben aber die bekannten Zellzwiebeln, eingeschlossen werden. Die einen derselben sind verkalkt, offenbar ältere Bildungen, die andern dagegen werden von feinkörnigen, protoplasmatischen, mit grossen Kernen versehenen Zellen zusammengesetzt. Es ist mir an diesen auffallend gewesen, dass die von Ammann genauer geschilderten colloiden Umwandlungen der Kerne fehlen; selten einmal findet sich eine bläschenartige, hydropische Auftreibung eines solchen von mässigem Umfange. Niemals wird durch dieselben die Zellsubstanz verdrängt, auf einen schmalen Ring reducirt, wie dies in normalem Drüsen jüngerer Individuen vorkommt. Ich halte diese Formen daher für jugendliche und habe mich bemüht, der Entwicklung derselben weiter nachzuforschen.

"Vorerst sei noch erwähnt, dass dieselben auffallend häufig in Längsreihen angeordnet liegen, welche durch schmälere Stränge ähnlicher platter Zellen zusammenhängen oder auch von einander getrennt sind. Auch ich habe in meinem Objecte keine offenen Gefässbahnen nachweisen können, welche mit diesen Zellzwiebeln zusammenhängen; wie diese Afanasiew . . . angibt. Dennoch halte ich einen Zusammenhang derselben mit Gefässbahnen für möglich, obwohl es mir nicht gelungen ist, jene verbindenden Stränge als solche zu erkennen.

"Indem ich nun aber nach Vorstufen solcher Formationen suchte, die doch nicht plötzlich von sich aus entstanden sein können, habe ich allerdings die Ueberzeugung gewonnen, dass sie in Gefässbahnen entstehen durch allmälige Anhäufung endothelialer Elemente, welche vielleicht zum Theil an Ort und Stelle entstanden sein mögen. Ich bemerkte, dass unter den Höhlräumen im Centrum der Acini auch solche von runder und ovaler Gestalt nicht selten vorkommen. Sie sind ausgekleidet mit einer Lage stark vorspringender, grosskerniger Zellen, die nicht immer genau aneinander schliessen. . . . Hiermit wäre nun wohl in plausibler Weise die Entstehung dieser Bildungen aus Endothelien erklärt, aber es bliebe noch übrig, die Natur der Hohlräume zu bestimmen, in denen sie sich bilden. Dieselben liegen mitten in einem Gewebe, dessen lymphatische Natur nicht zu bezweifeln ist; allein damit ist noch keineswegs bewiesen, dass sie ebenfalls im Lymphsystem entstehen.

"Leider ist eine directe Entscheidung durch die Verbreitung rother Blutkör-

That these conclusions of Klebs concerning the rôle of the thymus in the production of the disease were not seriously considered for long in the medical literature is a matter of historical fact. It does not seem that he himself pursued the subject farther for none of his subsequent publications (10) discuss it in any great detail. Pierre Marie (16) presented a brief theoretical note on the subject. Of the 265 post-mortem examinations of acromegalics analysed by Atkinson (2) in 1932 the thymus was described in 98. It was persistent in 54.6%, absent in 39.2%, and represented by fatty cells in 6.2%. In only five was it found to be hyperplastic or hypertrophied. Such statistical and anatomical studies are, however, of little import in a subject so intimately bound up with functional inter-relationships. Moreover, few of them can have been executed with the same fine detail with which Klebs attacked his case.

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perchen an dem vorliegenden Object ausgeschlossen, da dasselbe völlig blutleer ist. Dennoch muss ich mich für die Blutgefässnatur dieser Hohlräume entscheiden, indem ich mir vorbehalte, wenn es andere Arbeiten gestatten, diesen Gegenstand an anderen, zur Entscheidung geeigneteren Objecten zu bearbeiten.

"Was mich zu dieser Annahme bestimmt, ist der Umstand, dass jene erwähnten runden und ovalen, entweder mit wandständigen Zellen ausgekleideten oder mit grossen, lose liegenden Zellen erfüllten Hohlräume vielfach mit Kanälen in Verbindung stehen, von meist etwas engerem Lumen, zarter paralleler Begrenzung, welche mit platten Endothelzellen gleichmässig ausgekleidet sind. Dieselben sind offenbar Theile des Capillarnetzes, welches den Follikel durchsetzt; in unserem Falle ist dasselbe von enormer Mächtigkeit, einzelne Theile stärker erweitert als die anderen. . . .

"So bin ich der Meinung, dass die Blutgefässnetze der Thymusfollikel eine Bildungsstätte von Gefäss-Endothelien sind. Dieselben lösen sich entweder los, treten in den Blutstrom und werden fortgeschwemmt, oder sie häufen sich, wo sie sich besonders reichlich bilden oder Erweiterungen der Strombahn ihre Stagnation begünstigte, an und gestalten sich hier zu jenen Endothelperlen um.

"Somit erscheint auch mir die Entstehung dieser Bildungen als ein Zeichen reger Zellthätigkeit in der Thymus, ebenso in der jugendlichen Thymus (Ammann und Roth), wie in der Thymus solcher Thiere, bei denen die Drüse durch das ganze Leben hindurch unverändert bleibt, . . . wie endlich auch in unserem Fall für die pathologisch wachsende Thymus.

"Verfolgt man diese Anschauung weiter, so kommt man nothwendiger Weise zu der Frage nach den weiteren Schicksalen dieser Elemente, und es liegt nicht so fern, denselben für die Entwicklung des Gefässsystems eine weitere Bedeutung zuzuschreiben und sie als Angioblasten zu bezeichnen. . . .

"Wenn somit das vorliegende Material auch sehr spärlich ist, so mag immerhin die nachgewiesene Beziehung der Thymusdrüse zur Bildung von Angioblasten, sowie ihre Rückbildung mit vollendetem Körperwachsthum Veranlassung geben, diesen Beziehungen weiter nachzuforschen." [Excerpts from pp. 76-81.]

The Pituitary

Klebs did not miss the significance of hypophysial enlargement in the case of Peter Rhyner. He, as the following excerpts from his report indicate, was much impressed with the increased size of the gland. Moreover, he notes that this was a finding in similar cases previously reported, i. e., that of Brigidi (4). It is interesting to recall in this connection that although Pierre Marie reviewed the same case he did not comment upon the pituitary.

Examination of the brain in Fritzsche and Klebs' case revealed a tumor in the hypophysial region encroaching upon the optic nerves. The pressure of the tumor on the chiasma was clearly recognized by Klebs as the cause of the failing vision. He also noted that the hypertrophy of the gland was of special interest since it seemed to be constantly associated with generalized gigantism. In the case of Peter Rhyner the hypertrophy might be considered a morbid process or it might mark a simple increase of normal growth. This latter view is the one he finally accepted. The exact pathological descriptions and the interpretations he placed upon them were as follows:

"Regarding the nature of this tumor of the hypophysis, it belongs entirely to the anterior glandular part and has throughout the characteristics of simple adenoma or glandular hyperplasia. Its surface is smooth, uniformly spherical, and from the rather thick enveloping capsule numerous trabeculae and septa project into the interior, in their intermediate spaces, enclosing a soft mass, which, in chromic acid preparations, is somewhat brittle. In microscopic sections this same arrangement is also found in the finer subdivisions; finer trabeculae arising from coarser ones and dividing the soft substance into smaller lobules. The former contains wide blood vessels lined by a simple endothelial layer of large cells, surrounded by three to five layers of delicate spindle cells in which rod-like nucleii with protoplasmic walls are imbedded in an almost homogeneous, but slightly striated material. The abundance of the intercellular substance as well as the elongated oval shape of the nucleii deny the muscular nature of the layer which must, therefore, be considered the perithelium of the blood vessel itself.

"The spaces marked off by these trabeculae are filled with a large mass of cellular clumps separated from each other by small amounts of hyaline intermediate substance. A sharp, often double contour, a membrana propria, marks off this intercellular substance from the hollow spaces which are completely filled by loosely imbedded cells with large nucleii. These are sometimes round and sometimes angular, but never assume the regular

arrangement characteristic of most glandular cells. Not infrequently cells with two nucleii are found, such cells usually being larger than the mononuclear ones. The size of the individual alveolus varies greatly, depending primarily upon the number of cells which always fill the cavity completely. The smallest spaces hold but two or three cells but these may be merely the terminal pockets of larger alveoli. In general, it seems possible that all these cell-filled cavities intercommunicate, though only a few such communications can be demonstrated in microscopic sections.

"Accordingly we are dealing with a simple hyperplasia of the gland, the origin of which is to be explained by the great development of the vascular system. . . .

"In addition this hypertrophy of the hypophysis is of particular interest because apparently it regularly accompanies generalized gigantism. This may be, as in our case and that of Brigidi, the result of a pathological process or it may be the result of a simple physiological growth without special disorder, the latter fact being in agreement with Langer's descriptions of the skeletons of giants.

"Thus, the interdependence of the two processes is demonstrated and we may assume that the same basic causes and the same disturbances of development may call forth at one time the picture of simple increased growth and at another, serious pathological disorders. . . .

"The question must now be raised whether the hyperplasia of the pituitary must be considered as a part of the phenomenon of the increase of the total skull contents or if it be an exclusive phenomenon related either to its structure and physiological behavior or to its rich vascular supply. I would endorse the first hypothesis although with our complete ignorance of the function of the gland, it is difficult to reach any definite conclusion.

"As is well known, there are many varied forms of hypophysial tumors; adenomata, carcinomata, sarcomata, as well as vascular and cystic tumors, which may by their growth produce only mechanical disturbances as they press upon neighboring blood vessels, nerves, or brain substances. Thus, recently I dissected a case in which the tumor had even led to compression of a cerebral peduncle. But in none of these cases was a parallel alteration of brain substance, especially hypertrophy, as in our case, observed. The hyperplasia of the hypophysis must, therefore, be regarded as a phenomenon dependent upon the same causes as the cerebral hypertrophy. That a greater increase of the hypophysis than of brain substance occurred in our case depends upon the increased proliferative ability of the gland, the growth of the brain substance also being impeded by the increased growth of the hypophysis. Moreover, the increase in the size of the hypophysis does not reach such a high degree in all cases of general gigantism as in that of P. Rhyner. Thus, the gigantic skull of the Zurich collection . . . shows only a moderate widening of the sella turcica.

"Hence, while the enlargement of the hypophysis occurs regularly in

general gigantism this increase can be regarded as a phenomenon analogous to the hypertrophies of the other organs; it certainly does not assume a genetic relation to the enlargement of one of them." *

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* "Betrachten wir zunächst die Natur dieses Tumors der Hypophyse, so ergibt sich, dass derselbe ausschliesslich dem vorderen drüsigen Abschnitte angehört und durchweg den Charakter eines einfachen Adenoms oder einer Drüsenhyperplasie an sich trägt. Seine Oberfläche ist glatt, gleichmässig gewölbt, und von der ziemlich derben Umhüllungsschicht senken sich zahlreiche Balken und Scheidewände in das Innere, deren Zwischenräume von einer weichen, am Chromsäurepräparat etwas bröcklingen Masse ausgefüllt werden. Auf mikroskopischen Schnitten zeight sich dieselbe Anordnung auch in den feineren Unterabtheilungen wiederholt, indem von den gröberen Balken immer schmälere abgehen und kleinere Partien der weichen Substanz begrenzen. Die ersteren sind Träger weiter, von einer einfachen grosszelligen Endothelschicht ausgekleideter Blutgefässe, welche nach aussen von 3-5 fachen Lagen eines zarten Spindelzellengewebes begleitet werden, in welchem längliche Kerne mit Protoplasmahülle in eine fast ganz homogene, schwach gestreifte Grundsubstanz eingebettet sind. Die reichliche Zwischensubstanz, sowie die länglich-ovale Beschaffenheit der Kerne spricht gegen die muskuläre Natur dieser Schicht, die demnach wohl als Perithel dem Gefässe selbst zugerechnet werden muss.

"Die von diesen Balken umschlossenen Räume sind nun erfüllt von einer grossen Masse von Zellhaufen, welche von einander durch geringe Mengen hyaliner Zwischensubstanz getrennt werden. Ein scharfer, oft doppelter Contour, eine Membrana propria begrenzt diese Zwischensubstanz gegen die Hohlräume, welche gänzlich von locker eingelagerten grosskernigen Zellen erfüllt werden. Die letzteren haben bald eine runde, bald eine eckige Form, ohne aber jemals die regelmässige Lagerung der meisten Drüsenzellen darzubieten. Nicht selten finden sich auch solche Zellen mit zwei Kernen und sind diese dann gewöhnlich von bedeutenderer Grösse, als die einkernigen. Die Grösse der einzelnen Alveolen ist eine sehr verschiedene und hängt im Wesentlichen von der Zahl der Zellen ab, welche den Höhlraum stets gänzlich erfüllen. Die kleinsten Räume enthalten nur 2-3 Zellen, doch ist es wohl möglich, dass dieses nur endständige Ausbuchtungen grösserer Höhlen sind. Ueberhaupt macht es den Eindruck, dass alle diese mit Zellen gefüllten Höhlen miteinander in Verbindung stehen, was sich freilich an Schnittpräparaten nicht sicher feststellen lässt, doch sind einzelne solcher Communicationen leicht nachweisbar.

"Es handelt sich demnach um eine einfache Hyperplasie der Drüse, für deren Entstehung die reiche Gefässentwicklung eine hinreichende Erklärung bietet."

"Ferner aber hat diese Vergrösserung der Hypophysis noch dadurch ein besonderes Interesse, dass sie als eine, wie es scheint, constante Begleiterscheinung des allgemeinen Riesenwuchses auftritt, mag derselbe, wie in unserem Falle und demjenigen von Brigidi, unter dem Bilde eines Krankheitsprocesses oder sich allmälig und ohne besondere Störungen als eine einfache Steigerung des physiologischen Wachsthums darstellen, welche letztere Thatsache die von Langer gelieferte Beschreibung von Riesenskeletten erhärtete.

"Gerade hierdurch wird die Zusammengehörigkeit der beiden Erscheinungsreihen klar gelegt, und können wir annehmen, dass die gleichen Grundursachen und die

These statements in themselves seem too clear to comment upon them at any length. It may be well to realize that they were made at a time when the function of the pituitary was quite unknown and only about thirty years after Brown-Séquard had first suggested the functional significance of the endocrines.

Klebs' further speculations are concerned with other forms of gigantism, and their relationship to the disorder he had described. He finally concluded that there may be three types of growth disorders, all characterized by osseous hypertrophy but differentiated as follows: 1) subject to a marked increase in soft parts, 2) subject to a marked increase in the size of the internal organs, particularly, the hypophysis and thymus, and 3) subject to a marked increase in bony parts with an irregular increase in soft parts.

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gleichen Entwicklungsstörungen bald das Bild einfach gesteigerten Wachsthums, bald dasjenige einer schweren pathologischen Störung hervorrufen. . . .

"Es muss nun die Frage erhoben werden, ob diese Hyperplasie des Hirnanhangs als eine Theilerscheinung der Zunahme des gesammten Schädelinhalts betrachtet werden muss oder ob ihr eine exclusive Stellung zukommt, die entweder mit ihrer Structur und ihrem physiologischen Verhalten oder mit ihrem Gefässreichthum im Zusammenhang steht. Obwohl es schwierig ist, bei der gänzlichen Unkenntniss der functionellen Bedeutung der Drüse hierüber zu entscheiden, möchte ich mich doch für die erste Annahme aussprechen.

"Bekanntlich kommen die verschiedenartigsten Geschwülste der Hypophyse vor: Adenome, Carcinome, Sarcome und Blut- und Cystengeschwülste, ohne dass bei ihrer Entwicklung andere als mechanische Störungen hervortreten, indem sie einen Druck auf die benachbarten Blutgefässe, Nerven oder auf die Hirnsubstanz selbst ausüben. So secirte ich neulich einen solchen Fall, in dem der Tumor sogar eine Compression eines Grosshirnschenkels hervorgebracht hatte. Aber niemals ist in diesen Fällen irgend eine parallele Veränderung der Hirnsubstanz, namentlich nicht Hirnhypertrophie, wie in unserem Falle, beobachtet worden. Es kann somit die Hyperplasie der Hypophyse nur als eine von den gleichen Ursachen wie die Hirnhypertrophie abhängende Erscheinung betrachtet werden. Dass in unserem Falle augenscheinlich eine stärkere Zunahme der Hypophyse als der Hirnsubstanz stattgefunden, die letztere durch das stärkere Wachsthum der Hypophyse beeinträchtigt wurde, hängt wohl von der grösseren Proliferations fähigkeit der Drüsengegenüber der Hirnsubstanz ab. Ausserdem ist aber auch die Grössenzunahme der Hypophysis nicht in allen Fällen von allgemeinem Riesenwuchs eine gleich bedeutende, wie bei P. Rhyner. So zeigt der Riesenschädel der Züricher Sammlung . . . nur eine mässige Erweiterung der Sella turcica.

"So beständig demnach die Vergrösserung der Hypophyse bei allgemeinem Riesenwuchs vorkommt, so kann sie dennoch nur als gleichartige Erscheinung den Hypertrophien der übrigen Organe angereiht werden, sie steht keinesfalls in einer genetischen Beziehung zu der Vergrösserung eines derselben." [Pp. 64-66.]

Pierre Marie and Acromegaly

Pierre Marie reported the clinical aspects of what he recognized to be a distinct entity in two cases he had seen simultaneously in Charcot's clinic in the Salpêtrière (12). The features of the malady emphasized were the change in facies, the hypertrophy of the extremities and of the lower jaw, the large tongue, the parietal headaches, polydipsea and polyuria. These descriptions are lucid and simple, obviously those of an excellent clinician. They have often been reprinted (16) and are now easily accessible in English translation in Major's "*Classic Descriptions of Disease.*" (11)

Marie's interpretations, however, are seldom discussed. He reviewed eight somewhat similar cases which he had collected from the literature, and pointed out the similarities and differences upon which a differential diagnosis might be established. No suggestion was made of the relationship of the disease to the pituitary, neither of Marie's cases having come to the autopsy table, at this time. In only one of the cases which he reviewed from the literature was there mention of a pituitary tumor and Marie makes no comment on this finding. He speculates at length about the etiology of the condition but his speculations are clearly of less value than the simple clinical descriptions. He not only described the new entity but carefully delimited it from other conditions with which he thought it might readily be confused; i. e. myxoedema, Paget's disease (osteitis deformans) and the leontiasis ossea of Virchow. Small wonder that this description appearing in a widely circulated journal and under the newly coined name of *acromegaly* caught the eye of the medical world so that the literature was soon flooded with case reports. According to Sternberg (22), by 1890 Souze Leita, a pupil of Marie's who collaborated with him on several papers (16), had collected 38 cases; by 1891 Duchesnan added an additional 27; Collins by 1892 had a total of 83, and Sternberg himself had 130 by 1894 and 210 by 1897.

That Marie was unaware at the time he wrote his first paper of the genetic relation of the pituitary in acromegaly is quite evident. The following year Minkowski (18) reviewed the problem and commented at some length upon the finding of hypophysial tumors in so

many of the cases of the disorder. There is no hint of how much he was indebted to Klebs for this idea, though it is of some interest that he discusses Fritzsche and Klebs' finding and conclusions at some length. The chief argument he presented for the pituitary origin of the disorder was one of analogy, *i. e.*, if myxoedema was related to thyroid dysfunction, then acromegaly might be to pituitary dysfunction. Marie himself, oft refuted the pituitary theory (13) (15) and, according to Davidoff (6), who has told the story of acromegaly very well, continued to do so as late as 1911. Massalango (17) in 1895 suggested that the disease was not due to an undergrowth of the gland but it was only after the next two decades when the physiology of the gland became a subject of experimentation and its surgical removal a practical procedure that the present concept of acromegaly as an expression of hypophysial hyperfunction was built up.

Summary

In summary we note that Fritzsche presented a good clinical description of the disease we recognize today as acromegaly before Pierre Marie. Edwin Klebs from careful post-mortem studies on this case attempted to explain what appeared to him to be a disturbance of a normal growth mechanism in terms of a pathological abnormality and to relate this abnormality to the larger physiological problems of growth and development. Moreover, he related the disturbance to the endocrine glands. These are points which have long been overlooked in the history of acromegaly.

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THE HISTORY OF VAGAL INHIBITION *

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The Neurogenic Theory

The function of the cardiac vagus constitutes one of the oldest problems in physiology. The earlier history of this subject has been traced in a previous communication (79), in which it was shown how primitive neurogenic theories of the causation of the heart beat led investigators to stimulate the vagus nerves and to obtain clear evidence of slowing, block, and even arrest of the heart. This account terminated logically with the authoritative survey by Haller, who presented a new concept of the origin of the cardiac impulse based upon the inherent irritability of the myocardium itself. However attractive the myogenic hypothesis of Haller may appear at present, it was in fact only an interlude in the development of knowledge concerning the nature of the heart beat. Although Haller's theory was to have great influence in a later period, it is the neurogenic theory that provided the stimulus for the discovery of the action of the vagus nerves, for the interpretation given to this discovery, and for the extension to other aspects of physiology of the concept of inhibition developed for the vagus nerves.

It is not difficult to determine why the myogenic theory of Haller had such a poor reception, and was so rapidly replaced by a more adequate neurogenic doctrine, for the clue is found in Haller's own writings. Haller was faced with a dilemma he was unable experimentally to resolve. On the one hand, he could find no evidence that the cardiac nerves had any influence upon the heart, yet the effect of passions and mental turmoil seemed clearly to indicate that the central nervous system maintained some manner of control over the heart beat. His irresolution in the face of conflicting evidences is betrayed by the vacillating views he presents concerning the causation of the heart beat, and the influence of cardiac nerves. Thus at times he is willing to deny any nervous influence upon the heart:

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" For this reason the heart is subject to a different control than a voluntary muscle, for neither the will nor the nerves have any power over it " (66, vol. 5, p. 143). Or again: " One can finally add, that the force of the heart as such does not depend upon a definite class of nerves, and, what is more, a greater part of the force that sets the heart in motion does not originate in the nerves " (66, vol. 1, p. 915).

Nor could he entirely divorce himself from the thought that the nerves might play some part in cardiac contraction. In one place he suggested a trophic action to the nerves: " Since finally the excitability of muscles is present even in the absence of nervous influences, since the heart possesses properties similar to other muscles, one may clearly see from this how the contractions of the heart continue without the presence of nervous energy in an animal possessing neither head nor brain, nor spinal cord, or after it has been torn out of the thorax and cut in pieces. Since, however, the nervous force contributes much to the duration and magnitude of contraction, it is not improbable that the heart also receives a contractile force from the nerves, by virtue of which the contractions produced by its irritable nature becomes stronger and more rapid (66, vol. 1, p. 945).

In another place he was willing to yield an even greater function to the cardiac nerves: " Another conjecture is, that the heart is more irritable, because the sentient nerves of the heart being in close relation with the inner membrane of the heart, are stimulated by the contact of the blood; and that thence a more active movement arises than from irritation of the external portion of another muscle. The external surface of the intestines is, in a like manner, almost insensitive to stimuli, the internal most sensitive, and where irritated, continually excites movements. Can it be that the auricles are more excitable than the ventricles, and more apt for motion, because being so delicate the nerves are almost naked, and consequently exposed to the immediate stimulus of the blood? " (66, vol. 1, p. 936). This suggestion of a trophic function for the cardiac nerves was adopted by Tiedemann (150) who supported it by the observation that a muscle deprived of the nerve supply soon atrophies.

Haller's more zealous supporters were bolder than he and denied

without reservation that the cardiac nerves had any influence upon the heart. Among these was Fontana (55), who drew this conclusion from a study of the effects of certain poisons on the nerves and on the heart. The great anatomist, Soemmerring (147), suspected that the nerves of the heart, which he said were few in number and very indefinite, supplied in fact not the myocardium but the aorta and coronary arteries, and directed the investigations of his pupil Behrends (5) toward a solution of the problem. Behrend's dissertation, which won him a lasting fame, was boldly entitled "Cor nervis carere" and purported fully to substantiate Soemmerring's suggestions. It was not until several years later that Scarpa (140) was able to demonstrate by careful dissections that the cardiac nerves actually supplied the muscles of the heart and not the coronary vessels alone, but by then the neurogenic theory already had been renewed by the work of Legallois.

Legallois proposed to study the problem by the method which has since proven of such value in so many applications; that of progressive destruction of successive portions of the central nervous system. He successfully decapitated mammals and maintained the circulation by artificial respiration. When he then proceeded to destruction of the medulla and cord, he found that the animal failed to survive if the entire cord were destroyed. He concluded that the heart depends for its continued contraction upon the presence of at least a portion of intact cord, and assumed that the sympathetic nervous system, which was associated with the cord, was mediator of the influence it exerted upon the heart (104).

Legallois seems to have been unaware of the work of Fontana (55), who had destroyed completely the central nervous system of frogs without killing them, or of Whytt, who even earlier, had remarked that "the regular alternate contraction of the hearts of frogs persisted for five or six hours after decollation and destruction of their spinal marrow" (176, p. 7). Brachet (24), who witnessed the demonstrations of Legallois, was soon able to reproduce Fontana's and Whytt's results on frogs, and salamanders, and collected a number of reports of fetuses born without brain, medulla, or cord, which yet lived for some minutes after birth. At about the same time Flourens demonstrated that the entire brain and cord of mammals

could be destroyed if artificial respiration was maintained (54). Brachet concluded that the central nervous system was without influence on the heart, yet felt compelled to admit some nervous control, and suggested a mediation via the sympathetic nervous system, as had Prochaska, much earlier (153). At almost the same time Wilson Philip (131) reported similar experiments, with the same results, yet insisted that the whole question presented an as yet unresolved dilemma: Despite the evidence which proved that the heart could beat independently of the central nervous system there was reason to believe that the central nervous system did cause variations in the heart beat. Hall (63) and Magendie repeated these experiments and confirmed the results. Here it seems is the beginning of the recognition of the regulatory function of the cardiac nerves.

The new modification of the neurogenic theory which now developed formed the basis of all the physiological thinking until the close of the nineteenth century brought the great contributions of Gaskell. It probably had its origin in the ingenious reasoning of Unzer and Prochaska, who contributed two essential ingredients: (i) The heart beat is originated, and the coördination of cardiac contraction mediated, by reflexes, and (ii) the centres of these reflexes are situated outside the central nervous system, probably in the ganglia of the sympathetic. Unzer wrote: " Since the heart is composed of several muscles, and since these can be excited to action independently of each other, as has been shown in excised hearts, it is more probable that the stroke of the heart, or the combined action of all the muscular structures, is an indirect nerve-action of external impressions, they being reflected in the heart itself, than that it is simply a mechanical result of their direct nerve-actions; for in the latter case, the whole heart must be excited to action by an external impression which acts only on certain of its fibres—a conclusion opposed to the results of experiments " (153, p. 517). Concerning the sympathetic ganglia, he wrote: " The ganglia prevent the sensory impulses from the heart from reaching the brain, and serve as a reflex centre to cause the motion of the heart."

Prochaska proposed a similar mechanism: " For if we consider," he said, " that the minute and invisible nerves disseminated over the internal membrane of the heart and auricles, perceive the stimulus

of the inflowing venous blood, and although they cannot transmit the impression of that stimulus to the sensorium commune through the ganglia of the great sympathetic nerve, yet communicate it to the motor nerves of the ventricles and auricles, it follows that there is necessarily a consensus between the sensory nerves distributed on the inner membrane of the ventricles and auricles, which must take place either in the ganglia of the great sympathetic nerve or below them, in the communicating branches or plexuses of nerves. It seems probable, therefore, that besides the sensorium commune, which we conjecture to be in the medulla oblongata *medulla spinalis, pons variolii,* and *crura* of the *cerebrum* and *cerebellum,* there are special *sensoria* in the ganglia and plexuses of nerves in which external impressions ascending along the nerves are reflected, that need not ascend all the way to the *sensorium commune,* to be reflected thence" (153, p. 438).

The next development in the theory which was to attribute the rhythmic beat of the heart to the discharge of nerve cells in the heart itself came from the work of Bichat, which taught that the ganglia of the sympathetic acted not only as centers of reflex activity, but were also capable of independent discharge, and thus might act as the automatic centres of the rhythmic beat of the heart; "The nerves having the ganglia cannot transmit any cerebral action, because we have seen that the system of nerves originating in these bodies ought to be considered as completely independent of the cerebral nervous system; that the great sympathetic does not at all take origin in the cerebrum, the spinal cord, or the nerves of animal life; that this origin is exclusively in the ganglions; that this nerve does not, properly speaking, exist; that it is nothing but an ensemble of as many little nervous systems as there are ganglions, which are the particular centres of the organic life, analogous to the great and single nervous centre of the animal life, the brain (15)."

Prochaska proposed an ingenious theory to account for the continued beating of a heart torn from the body: "That the *vis nervosa* can remain a long time in the nerves, independently of the brain, seems to be proved by the state of paralytic limbs, the nerves of which are deprived of all connection with the brain on account of some preternatural compression, and yet for a long period the

paralysed muscles are convulsed by the stimulus of the electric spark. The *vis nervosa* of the spinal cord, and of the nerves given off by it, remained in a case after atheroma of the medulla oblongata, pons variolii, and entire cerebellum had destroyed the connection between the spinal cord and a dropsical brain. Moreover, the *vis nervosa* not only continues in a cord and nerves long separated from their connection with the brain, but exists in nerves that never had any connection with the brain whatever. This is proved by the histories of acephalous foetuses, which live the whole period of intra-uterine life, were nourished, increased in growth, and when born evinced no obscure signs of vitality, without a brain, and by the sole *vis* of the nerves and spinal cord, if the latter was not defective. Animals which have nerves but no cerebrum also demonstrate the same fact.

From these facts it is obvious, that the *vis nervosa* remaining in the nerves after the severance of their connections with the brain, must be considered as the cause whereby the heart was able to continue its movements in the experiments instituted by Haller and other distinguished men, after the brain and cerebellum were destroyed, the heads cut off, and even all the nerves of the heart divided" (153, p. 398). This theory Whytt also proposed, and rejected, as follows: "For although the spirits remaining in the heart, should be supposed to occasion a few contractions of the muscle, yet they cannot be sufficient for producing several thousand distensions of its hollow fibres." Müller also made the same suggestion (118).

When finally, small ganglia were found in the heart itself, the evidence seemed to be complete and they were immediately accepted as centres of the heart beat. Retzius had noticed collections of tiny ganglia along the course of nerves after they had emerged from the main ganglia (138), but it was Remak who reported that ganglia could be seen near the A-V junction of the heart of the calf, sheep and pig with the naked eye (135-137); "Because of this one must assume that the movement of the heart depends especially upon sympathetic nerve fibres, and thus upon the ganglia. This view receives a confirmation amounting almost to certainty through a second observation, which I take this opportunity to report, that along the peripheral branches of the cardiac nerves there are to be found

countless tiny, almost microscopic ganglia lying partly superficially and partly buried in the muscle fibres" (135).

The significance of this discovery for the neurogenic theory was instantly appreciated by Remak himself, who found it an explanation for the continued beating of an excised heart. Inspired by it, Ludwig (106), and later Bidder (16), were able to find other collections of ganglia in the hearts of both amphibia and mammals, while Bidder (16-20) and Volkmann (154-159) rapidly formulated the major outlines of the neurogenic theory (See also 59, 101). This theory, which attributed the origin of the heart beat to spontaneous or reflex activity of the cardiac ganglia, and the transmission of the cardiac impulse to the intracardiac nerve fibres originating in the ganglia, is stated thus briefly by Volkmann:

> The central organ of the heart-beat I assume to be the ganglia found by Remak, partly because they are the only structures in the system of the cardiac nerves which are to be distinguished from the universally present nerve fibres, and partly because they contain the cell bodies which seem to be essential to the function of central organs.
>
> Concerning the disturbances of contraction brought about by incisions into the substance of the heart, it seems to me that the ganglia as well as the intercommunicating nerve-fibres constitute a unified system; and provide the structural basis for the coördinating principle which binds together the contraction of countless muscle bundles into a purposeful unity and sequence. Those disturbances which result from cuts made into the heart wall, are apparently results of the damaged nervous communications, and not the results of cut muscles, for the latter could only give rise to an altered form of contraction and not a disharmony of rhythm.
>
> Apparently the ganglia, as the sources of the motor energy, are by virtue of their intercommunicating fibres capable of reciprocal action, so that the action of any single one influences all the others, and is in turn regulated by all of them. If then the communicating fibres are cut, each ganglion operates by virtue of its own inherent energy. (157)

Vagal Inhibition: Discovery

The firm establishment of the neurogenic doctrine by Remak, Bidder, and Volkmann coincided with the discovery that stimulation of the vagus nerve would slow, and even stop, the heart. With this discovery the brothers Ernst Heinrich and Eduard Friedrich Weber are indissolubly and rightly associated. Although earlier workers

had probably obtained vagal effects, they either neglected or misinterpreted them. They were unable because of the deficiencies of their methods of stimulation to obtain consistent results, and were forced to discount the value of any results they did obtain. Fontana, who probably first stimulated the vagus electrically, obtained no results whatever. Volkmann, however, reported in 1838 in a paper on the structure and functions of the cranial nerves in the frog (155):

> Nothing is more unusual than the influence of the vagus on the movements of the heart. I destroyed the brain and the cord of a frog with a stylet, so that all reflex movements were prevented, removed the forward extremities and the pectoral girdle with the least possible damage to the great vessels, and exposed the visceral branch of the vagus. Approximately a half an hour after the death of the animal I began to stimulate the vagus by means of a galvanic pile of eight pairs of plates, each 4 inches square, incessantly making and breaking the circuit. Immediately before the experiment the heart beat 30 times a minute, in the second minute, after onset of stimulation 33, and the same 3, 4 and 5 minutes later. The experiment was then stopped. Three quarters of an hour after death of the animal the heart still beat 29 times a minute. Stimulation being then commenced, the heart beat 11 times the second minute, 31 in the third, and 34 in the fourth. In the second minute, when only 11 beats were counted, there was not simply slowing, but an unmistakable arrest, during which five to six successive heart-beats were dropped. It is noteworthy that during the second minute the heart exhibited unusually weak contractions, while they were of normal size before and after this period. The experiment was tried a third time two hours after death. Before the application of the galvanic stimulus, 25 strong contractions at regular intervals were noticed in a minute. After the vagus had been stimulated for a minute, I counted in the second minute 26 beats, and in the third only 16. The intervals between the extremely weak contractions seemed in general not to have changed, but 8 to 10 pulsations were completely absent, so that the heart remained for almost half a minute at rest. A full quarter hour later the fourth series of experiments were performed. The heart showed 20 pulsations in a minute. After the vagus was stimulated there appeared in the second minute 4 almost unnoticeable contractions at the usual interval, and then the heart stood completely still for one and one half minutes, and then gave a single weak contraction. The galvanic stimulation was suspended, and there then occurred 26 strong and regular contractions in a minute. Stimulation of the vagus also increased the peristaltic movements of the viscera, and produced them after they had stopped.

After attempting without success to repeat similar observations on dogs and cats killed by a blow on the head, Volkmann later was led

to reject the positive evidence of his experiments on frogs, and being occupied with his studies on reflex phenomena, did not repeat them (156).

It has not been possible to ascertain positively whether the brothers Weber were aware of these experiments, when, a few years later, Eduard Weber began their classical studies. In a discussion of priority raised later by Heidenhain, Eduard Weber indicated that he was then aware of Volkmann's experiments, as well as his rejection of their implications, but he did not state that he knew of them at the time he began the stimulation of the vagus (167). In view of the enormous interest in Volkmann's reports as they appeared, and because Volkmann was a favorite pupil of Ernst Heinrich Weber, and had, in fact, left Leipzig only the year before, it is probable that Volkmann's experiments were well known to the Webers, and may even have served as the starting point of their own. Some further slight evidence that the Webers had seen Volkmann's report is to be found in the fact that Ernst Heinrich had published a paper in the same volume of the journal in which Volkmann's account of vagal stimulation appeared (168). The essential factor, however, in the success of the Webers' experiments was not the more or less successful experiments of any predecessor, but their stimulating device. The work of Faraday had paved the way to the development of an electric generator in which coils of wire revolved between the poles of magnets. By means of suitable commutators a series of electric waves of similar polarity could be produced, at a frequency determined by the number of coils and the rate of rotation. This device was soon adopted by physicians for stimulation of paralysed limbs, and other medical uses.

It cannot be determined with accuracy who was the first to construct such a machine. Spurgeon built one in 1827, and in Germany Pixii and Stöhrer were given credit for similar machines. In 1838 Clarke described an apparatus practically identical with the Webers', in which metallic commutators were employed instead of the usual mercury type (35). His illustration is significant in that it depicts special electrodes to be soaked in vinegar and used for medical application of the current. This apparatus was first employed in Physiology by Eduard Weber (169), who in 1836 wrote his in-

augural dissertation on *Quaestiones physiologicae de phenomenis galvano-magneticis in corpore humano observatis,* and was thus interested in the physiological application of electricity (165). Both Ernst Heinrich and his equally distinguished brother Wilhelm Eduard must have influenced their younger brother Eduard Friedrich. Ernst Heinrich, the eldest brother, was already professor of anatomy at Leipzig, and in addition to fundamental anatomical investigations, had laid the foundation for modern theories of wave motion, and terrestrial magnetism in a series of classical investigations in collaboration with Wilhelm. Wilhelm, in turn, had already begun the fruitful association with Gauss, and together the two had built one of the earliest telegraphs.

Eduard had employed several of the formulations of Gauss in his dissertation, in which he measured the electrical resistance of the living human body, and then with Wilhelm wrote the classic *Die menschlichen Gehwerkzeuge*, in which the mechanical problems of locomotion were carefully and quantitatively considered. They also prepared a primitive motion picture apparatus, in which drawings of figures at progressively advancing stages of locomotion passed a slit placed before the observers' eye, and gave the impression of locomotion. Continuing then to the more intimate physiological problems of muscular motion, and having learned of the stimulating device from Wilhelm, Ernst Heinrich, or from Gauss himself, Eduard began his great monograph on " Muskelbewegung " for Wagner's *Handbuch* (166). In the course of these studies the inhibitory action of the vagus nerves was discovered. While first published under joint authorship of Eduard and Ernst Heinrich, it was Eduard who later pointed out the significance of the discovery and applied to the phenomenon the name inhibition (*Hemmung*). Ernst Heinrich later gave his brother the entire credit for the discovery in a note in *Müller's Archiv* entitled " Eduard Weber's Entdeckungen in der Lehre von der Muskelcontraction," and Ludwig did not mention vagal stimulation in his obituary of Ernst Heinrich many years later.

While much more remains to be learned concerning the details of the discovery, it is well known that the first public announcement of the discovery was made at Naples, in 1845, and an account of it published several months later in Omodei's *Annali universali de*

medicina, and again in a supplement to the *Archives générales de médecine* (170). Almost simultaneously Budge published accounts of stimulation of the vagus in frogs (30, 31). He first stimulated the medulla of the frog and noticed that while the skeletal muscles were thrown into convulsion, the heart was arrested. When the spinal cord was then stimulated below the medulla, convulsions of the skeletal musculature occurred as before, but the heart continued to beat. Finally he observed that stimulation of a vagus nerve also arrested the heart. This discovery by Budge seems to be entirely independent of that of the Webers' (33, 34), although Ernst Heinrich Weber stated that Budge learned the use of the galvanomagnetic stimulator from Eduard Weber.

Shortly after this Claude Bernard described vagal arrest, but did not claim it as original, and probably was merely repeating the Webers' observations. As word of the discovery spread, a great number of investigators became interested, and most were able to demonstrate that stimulation of a single vagus was sufficient to arrest the heart. Eduard Weber, who at first had thought that the action of both vagi was required, soon corrected his earlier error, and agreed with the other workers that a single vagus only need be stimulated to arrest the heart. Some time later Mayer demonstrated that the right vagus was more powerful in arresting the turtle heart than the left, and Masoin (113) and Arloing and Tripier (1) extended these observations to mammals. Gaskell later found that stimulation of the vagus also produced block at the A-V node, and this was soon confirmed by Chauveau.

Interpretation

It is but natural that existing physiological principles should have served as a basis for attempts to explain the nature of the remarkable influence on the heart exercised by vagal stimulation. At least five theories, with certain variations in some instances, were proposed. Only one of these has survived after decided modifications, nevertheless all have contributed to a greater or lesser degree to the development of physiological thought (115).

A. Budge's theory of tetanus

The simplest of all theories was that first proposed by Budge (32). He assumed that the vagus was in fact the motor nerve of the heart, and that faradic stimulation of that nerve threw the heart into tetanic contraction, in which, as in a tetanized muscle, individual contractions could not be detected and the organ thus appeared to be at rest. This suggestion received no support from any other workers, and was quickly abandoned by Budge himself, who soon recognized that the heart stopped in diastole when arrested by the vagus, and that while it was so arrested, it was in fact responsive to external stimulation. This observation was also made by Eckhard (47-49). Budge's suggestion persisted, however, and the question whether the heart could or could not be thrown into tetanus came to attract the attention of many physiologists. Eventually, through the interest of Bowditch and of Marey, it contributed to the discovery of the refractory period.

B. Brown-Séquard and coronary constriction

Marshall Hall had some years earlier suggested that sudden closure of the coronary arteries might cause immediate arrest of the heart, and experiments performed by Erichsen, acting on this suggestion, seemed to bear out his contention. Brown-Séquard had himself played an active part in the discovery of vasoconstrictor fibres to blood vessels, and synthesizing this material with Behrend's theory that the vagus supplied only the coronary arteries, suggested (i) that the vagus contained the vasomotor fibres to the coronary arteries, (ii) that when the vagus was stimulated, the coronary arteries were constricted, and thus the heart was deprived of its normal supply of blood, and (iii) the cardiac ischemia thus produced was responsible for the slowing and arrest of the heart (25, 26).

This theory, like that of Budge's received only passing attention, being supported temporarily only by Goltz (60). It was later disproved by the work of Panum (124), who plugged the coronary vessels by injection of lycopodium spores and found that the heart did not immediately stop. He further showed that while the heart continued for a time to beat with plugged arteries it was still sus-

ceptible to the arresting influence of vagal stimulation. Panum's studies and especially his method of injecting particles into the blood stream to arrest the circulation in certain vessels were to have a great influence upon later work on coronary occlusion and artificial embolism, so that again an untenable theory of vagal activity had unexpected and not unimportant ramifications.

C. The "back-firing" theory of Wallach

An ingenious theory of Wallach postulated that stimulation of the vagus "back-fired" impulses down the sensory fibres of the vagus and blocked the afferent impulses from the heart which were responsible for the reflex which produced the heart beat (160). Since simply cutting the vagus, which equally effectively blocked afferent impulses, did not cause arrest, he suggested that the electricity had some antagonistic effect on the afferent nerves: "One artificially imposes against the centripetal action a centrifugal one, and the result of this is cessation of reflex activity. Of course the same phenomenon does not follow the simple section of the vagus trunk on the contrary, the heart continues to beat, although in an altered manner. This has the following explanation: First the cardiac nerves form anastomoses with each other, which are linked together by countless ganglia, and these ganglia suffice although not permanently, to reflect the centripetal activity onto motor fibres. Second, in simple section of the nerve, the antagonistic effect of electricity on the centripetal activity is lacking."

D. Schiff's theory of "exhaustion"

Like Budge, who later became one of his most active proponents, Schiff based his reasoning upon the assumption that the vagus was the normal motor nerve of the heart, and attributed the arrest following vagal stimulation to the exhaustion of the motor nerve because of excessive stimulation. In support of this theory he reported experiments which showed that weak stimulation accelerated the heart while strong stimulation arrested the heart. Similarly, he claimed, weak stimulation of its nerve would throw a skeletal muscle into tetanus, while with excessive stimulation it would fatigue and fail to

respond (141-143). Schiff's proposal was vigorously opposed by most of the physiologists of the day, and supported by only a few, of whom Moleschott (82, 116) was the most active. Von Bezold (12) and Pflüger (129-130) led the opposition, and not only failed to confirm his experiments, but raised insurmountable obstacles to his theories. They maintained that cutting the vagus put it out of activity far more effectively than exhaustion, and Pflüger blocked the intact nerve by an ascending constant current, yet the heart continued to beat (50). Finally, the discovery of reflex vagal discharge made his theory untenable in the minds of most workers.

What Pflüger and all his opponents failed to recognize, however, was the distinction, which Schiff tried to make clear, between what he called " exhaustion " and " fatigue." " Exhaustion " according to him occurred only through physiological activity, while fatigue came about through the action of deleterious agencies. In modern terminology what Schiff meant by " exhaustion through activity " must be " refractory period," and his theory of exhaustion as a cause of vagal arrest probably meant " Wedensky inhibition." There is much evidence that this is in fact the exact significance of his expressions. In the first place he insisted that the " exhaustion " of the nerve was propagated along the entire nerve to its most remote endings, while " fatigue " was confined to the region between the electrodes affected by the electrical current, and that this is why the cut nerve differed from the " exhausted " nerve. Secondly, he had clearly in mind an idea of a relative refractory period, as shown by the following:

> The exhaustion is also often only a relative condition, applying only to certain degrees and kinds of stimulation. A nerve, which to a certain stimulus is exhausted, is not for another, more intense, but within these limits the paralysis is total, extending into the utmost periphery of the nerve. A cut nerve on the contrary, lacks only the stimulation, but its reactivity is retained unchanged.

Proceeding then to the theory that the rhythm of the heart is due to alternate restitution and exhaustion of excitability he actually demonstrated that there are periods when the beating heart is unexcitable while at other times during diastole it is excitable. Here too he recognized a relatively refractory period: " Here therefore the needle

must have touched the heart in the short period of its cycle during which the diastolic pause can be shortened by a strong stimulus, because at this time the cardiac nerves are again reactive to a strong but not yet to the normal stimulus" (143).

Transferring these ideas bodily to the skeletal muscle-nerve preparation he clearly demonstrated, and what is more, approached very closely the explanation of Wedensky inhibition. His first experiment was simply to stimulate a frog's sciatic at a rapid rate for several minutes, until the contractions became uneven, and much diminished in force. "If now the stimulation be continued at the same strength, the exhaustion reaches a degree, in which the twitching and all movement of the foot ceases entirely; the foot lies there as though dead. When one stops the stimulus only for a single moment, by arresting the hammer for a sixth of a second or less, there appears when stimulation is resumed a new twitch, *but only a single one*, and then all is again quiet, no matter how long the current passes through the plexus."

To imitate more closely the situation in the heart, he set up another preparation in which a tetanic stimulus was applied to the cut end of the nerve near the plexus, and single break shocks sent in at rhythmic intervals through electrodes placed further down the nerve. Here again he was able to demonstrate that when the muscle had relaxed under the influence of the tetanic stimulation of the plexus, the nerve was inexcitable through the second pair of electrodes as long as the tetanic stimulation continued. When this was stopped, even for an instant, the nerve immediately became excitable along its course.

Thus it can be seen that Schiff's interest in the mechanism of vagal action led him to postulate the existence of a refractory period in nervous tissue, to perform experiments which demonstrated its presence in the heart, to reason that a form of inhibition based on the action of excessive stimulation in producing a state of continued refractoriness should be found in skeletal nerve-muscle preparations, and finally, actually to demonstrate the existence in such preparations, of what is known as Wedensky inhibition. The absence of graphic recording devices prevented Schiff from obtaining objective evidences of these phenomena, and from analysing them in greater detail, and they are all rediscovered later by those whose names are

commonly associated with them (172, 173). It is unfortunate that the failure of Schiff's theory to apply to the vagus should have prevented the recognition of the parts of his work that were, essentially correct.

In England, Lister for a time at least upheld Schiff's theory. In a letter published in the *Proceedings of the Royal Society,* he wrote:

> The view which has been advanced by Pflüger, and I believe very generally accepted, viz that there is a certain set of nerve fibres, the so-called 'inhibitory system of nerves' (*Hemmungs Nervensystem*), whose sole function is to arrest or diminish action, seemed to me from the first a very startling innovation in physiology; and you may recollect my mentioning to you in conversation, when in London last Christmas, my suspicion that the phenomena in question were merely the effect of excessive action in nerves possessed of the functions usually attributed to them. On further reflection upon the subject, the consideration of the contraction produced in the arteries of the frog's foot by a very mild stimulus, as compared with the relaxation of the vessels caused by stronger irritants acting through the same nerves, confirmed my previous notions. For I could hardly doubt that the cause of quiescence of the heart or intestines on irritation of the vagus or splanchnic nerves was analogous to that of arterial dilatation in the web, and that, provided a sufficiently mild stimulus were applied to the so-called 'inhibitory nerves,' increased action of the viscera would occur, corresponding to the vascular constriction" (105).

Submitting this to test in rabbits, he found that "on the whole, the experiments appear to show that, in a healthy state of the nervous system, very gentle irritation of the vagus increases the heart's action, while a slightly stronger application diminishes the frequency and force of its contractions." From this he concluded that,

> these and other considerations, to which the already excessive length of this letter forbids me to allude, induce me to think it safest in the present state of science to regard as a fundamental truth not yet explained, that one and the same afferent nerve may, according as it is operating mildly or energetically; either exalt or depress the function of the nervous centre on which it acts. It is, I believe, upon this that all inhibitory influence depends, and I suspect that the principle will be found to admit a very general appreciation in physiology.

E. Eduard Weber's theory of inhibition

To no less an extent than Budge, Brown-Séquard, and Schiff, Eduard Weber sought to explain vagal arrest of the heart in terms

of established physiological knowledge. Naturally his own scientific background played an important part in determining his thinking concerning the apparently new phenomenon he and his brother had uncovered. For several years he had been actively interested in the physiology of muscular motion, not only from the standpoint of dynamics, which largely occupied his classic monograph with Wilhelm, but also in the intimate physiology of the neuro-muscular phenomena involved in the production of motion. In the neurophysiological literature at that time certain observations had already come to light which clearly indicated the existence of inhibitory processes in the central nervous system.

The power of the will to stop motion as well as initiate it had long been recognized, but with the recognition of the reflex nature of many processes, its implications were much more clearly stated. Marshall Hall (65) and Müller (118) especially recognized that removal of cerebral influence enhanced the reflex activity of the cord, and Hall made use of this observation to distinguish between paralyses due to cord lesions and those due to injury to peripheral nerves. Several writers had even this early formulated theories to explain how the brain exercised this control over the activity of the reflex centres in the cord (3). Among these was Volkmann, who suggested that the " attention " of the brain made the ascending fibres in the cord better conductors, permitting them to drain off the major portion of the inflowing stimulation, thus diverting it from local reflex channels. He likened this to the action of a lightning-rod, which by virtue of its greater conductivity, drains lightning away from the wood and stone of the building (154). What is more, Volkmann recognized this principle in the action of nerve centres controlling phasic movements, such as walking and the heart beat, and used the term inhibition (*Hemmung*) in reference to it:

> On stimulation of nerves, or of the nerve centres, contractions take place which are maintained as long as the stimulus is applied, so that immobility results from the maintained tension. If on the contrary one stimulates a motor nerve reflexly phasic movements occur despite uninterrupted continuance of the stimulation. Contraction and relaxation alternate, and one is struck by the impression of usefulness. It is fully demonstrated by many varied experiments that stimuli which primarily affect afferent nerves, are transmitted to the motor nerves only via the centres. Since with no other

kind of stimulation does it happen that a constant stimulus produces movement (*i. e.*, an alternation of contraction and relaxation), and since in all other circumstances a continuous stimulus produces continuous contraction, the central organ must contain the mechanism for the former circumstance. There is no doubt but that continuous stimuli would throw the muscle into uninterrupted contraction did not the central organ ***inhibit*** (***hemmen***) its influence at the right time, and thus permit a purposeful movement.

From the above, the continuous stimulation which affects these nerves, would cause a constant contraction in the viscera of the thorax and abdomen, were the motor stimuli not interrupted by central organs which periodically ***inhibit*** (***hemmen***) and purposefully regulate them (158).

Weber was well aware of all of this, and attributed the action of the vagus to an exactly similar inhibition of the motor centres of the heart:

The fact that activity of an involuntary muscle can be inhibited by the influence of the nerves that supply it, is new, and would be entirely without parallel, if we considered the vagus nerves as the actual nerves going to the muscle fibres of the heart, and the inhibition as the direct result of their action on these. We have, to be sure, examples of similar inhibition of the involuntary action of animal muscles; but this takes place not because the nerves are activated, but because they cease to function because of influences on the spinal cord, which is at the basis of their activity.

Just as the inhibitory activity in the skeletal muscles acts primarily on the spinal cord, so the inhibitory action of the vagi does not act directly on the muscle fibres, but primarily upon those structures which are responsible for cardiac activity, but which here are found in the substance of the heart itself. Resumption of beating with prolonged stimulation is due to fatigue of the vagus and escape (*Befreit*) of the motor nerves of the heart (166).

The theory of Weber, therefore, did not in reality offer an explanation of the mechanism of vagal inhibition, but, rather called attention to a phenomenon already recognized in the central nervous system, and applied it *in toto* to the heart. He did, however, for the first time, present a unified concept of this new factor regulating nervous reaction, showed that it was capable of wide application in physiology, and it is from his work that the doctrine of inhibition can rightly be said to have received its main impetus.

The concept of inhibition was returned to the central nervous system, where it had in reality originated, largely through the efforts of Setchenov (144) and Goltz, who not long after the Webers' discovery had exclaimed irritably, "Let us resist this flood of inhibitory

nerves" (61). In peripheral tissue inhibition was soon found again by Pflüger (126-128), who, following a suggestion of Carl Ludwig (106), and avoiding errors in technique which had earlier prevented Haffter (62) from obtaining positive results, discovered that stimulation of the splenchnic nerves inhibited the movements of the intestines. This was soon confirmed by Kölliker (95). Pflüger also insisted on the strict application of Eduard Weber's hypothesis that the nerve did not directly inhibit the muscle fibres, but rather inhibited the intramuscular ganglia which constituted the motor centres of the muscle. Two observations of Bidder also contributed greatly to the acceptance of Weber's explanation. First he noticed that the inhibited heart would respond to artificial stimuli, which was accepted as evidence that the heart muscle itself was not inhibited. Second, he observed in histological preparations that the fibres of the vagus nerves ended predominantly in the ganglia (17), where, according to Weber's theory, they would be expected to terminate. This agreed with von Bezold's demonstration that atropine paralysed the vagal endings in the heart (13).

Thus the entire structure of the neurogenic theory, upon which Eduard Weber based his explanation of vagal action, came to be greatly strengthened by it, and for almost half a century dominated physiology. Two reports of this period, however, served to mark the way from Haller to Gaskell. The first was the observation by Purkinje, who reported that "on the inner walls of the ventricles of sheep's hearts, I saw, first with the naked eye, a network of fine, flat, jelly-like, fibres lying just under the endocardium, which continued partly into the papillary muscles and around other fibre bundles, and partly bridged over single folds and fissures of the heart wall" (133). The second was the rediscovery by Bidder of the remarkable intercommunications between individual fibres of the myocardium, and the suggestion he made that the muscle fibres themselves might serve to transmit the cardiac impulse. When then, Gaskell laid the foundation for a new myogenic doctrine, and at the same time elucidated the nature of the cardiac ganglia, the way became clear for the development of a more complete understanding of vagal activity. In this new knowledge the pace-maker, and the A-V node have replaced, to be sure, Bidder's and Remak's ganglia, but the insistence of

Eduard Weber on the unity of the inhibitory activity in the heart and nervous system remains one of the most stimulating concepts in physiology.

Reflex Activity of the Vagus

Elucidation of the function of vagal discharge in the intact animal followed closely upon the discovery of the effect of stimulation of the nerve itself and the establishment of inhibition as a new category of nervous activity. The essential development in this respect consisted in the discovery that the nerve could be stimulated reflexly. Historically, there were three main developments: (i) reflexes mediated through the abdominal sympathetics in frogs; (ii) trigeminal reflexes in mammals; and (iii) reflexes in response to alterations in arterial blood pressure.

A. Goltz and the "Klopfversuch"

The first suggestion of a reflex vagal discharge is found in experiments performed by Budge, who stimulated the abdominal viscera of frogs and obtained slowing of the heart when the nervous system was intact (32), although Hall had earlier made the same observations in an eel with brain and cord destroyed (63, p. 160). Not long afterward Brown-Séquard discovered that crushing the semilunar ganglion in mammals caused arrest of the heart, and that the experiment failed if either the splanchnic nerves or vagus nerves were cut (27). This reflex, he thought, might account for the sudden death sometimes produced by blows on the abdomen (*i. e.*, solar plexus blows).

These observations, and another of Budge's, in which he produced arrest of the frog's heart by dashing the animal to the floor with considerable violence, led in 1863 to the description of Goltz' "Klopfversuch" (61). By tapping with the handle of a scalpel on the abdomen of an intact frog, Goltz was able to produce slowing of the heart, which failed to appear following vagotomy. The source of this reflex lay in the abdominal viscera, and not the abdominal wall. Goltz was not able to elicit the response by stimulation of the sympathetic fibres to the viscera, but this Bernstein soon demonstrated (7-10).

B. Reflex stimulation of the vagi via the trigeminus

While Goltz was thus able to demonstrate that the vagi could be stimulated reflexly, the fact that the experiment succeeded only in frogs permitted no conclusions to be drawn concerning the circumstances in which vagal discharge occurs in mammals. One of the earliest demonstrations of reflex vagus discharge in mammals, and strictly applicable to man, was found in the effect on the nasal mucous membrane of irritating vapors. Search for the cause of sudden death during chloroform anaesthesia led Dogiel (45) to make a thorough study of the various stages of chloroform narcosis, and to observe during the earliest stages a pronounced slowing of the heart, which occasionally amounted to a temporary arrest. These phenomena did not, he believed, result from the specific action of chloroform on the heart, but were due rather to a reflex discharge of the vagus evoked by stimulation of the olfactory nerve, since fumes of ammonia, alcohol, ether, acetic acid and other substances gave the same results. When, on the contrary, these materials were inhaled through a tracheal cannula, and did not come in contact with the nasal mucosa, no vagal effects were produced. Three years later Rutherford (139) observed that if the vagi were cut the heart showed no decrease in rate when irritating vapors were inhaled. A year after this Kochmann was able to demonstrate that the afferent pathways were contained in the trigeminus nerve and not in the olfactory nerve (58, 89, 94).

C. Vagal activity in response to changes in blood pressure

In Stephen Hales' famous hydrostatic experiments on the blood pressure in horses, the rate of the heart beat was carefully noted at every state of the experiment, and he clearly recognized that the heart-rate increased as the blood pressure dropped. This relationship between heart-rate and blood pressure he believed might explain the rapid hearts of children, and adults who had lost blood. From a consideration of Hale's observations Marey (110-112) thought that a general law might be formulated; that of the constancy of the work of the heart. Thus the rate would stand in inverse proportion to the resistance to be overcome by each beat. The experiments of Hales

he accepted as partial proof and then proceeded to determine whether changes in the opposite direction, namely increase in blood pressure, would cause slowing of the heart. This he was in fact able to demonstrate. He first evoked changes in blood pressure by altering the position of the body in human subjects, and then produced more pronounced increases by temporary occlusion of voluminous arteries both in man and in experimental animals. By this procedure he was able to produce marked slowing which was especially well illustrated by experiments carried out at his suggestion by Chauveau, who occluded the abdominal aorta of an intact horse and obtained sphygmographic records of the ensuing slowing. Nicoladoni (121) and Israel (84) were the first to note the marked slowing of the heart produced by occluding the artery leading to an arterio-venous anastomosis in man. For some reason, however, Marey neglected to consider the possibility of the participation of the vagus and assigned the slowing to a direct effect of the blood pressure on the heart.

Nearly three-quarters of a century elapsed before a final answer could be given to this problem; in that time three major developments could be traced: (i) recognition of the rôle of the vagus in the alteration of the heart rate caused by blood pressure changes; (ii) the discovery of the depressor nerves, and (iii) the elucidation of the function of the carotid sinus.

In 1863 von Bezold (12, 14) revived the question proposed by Legallois and attempted to determine the influence exerted by the spinal cord on the heart. Using curarized mammals, with the vagi and sympathetic chain cut in the neck, he found that stimulation of the thoraric cord provoked an elevation of the blood pressure and an increase in the rate of the heart. He concluded that the rise in blood pressure was due to the action of the spinal cord via the sympathetics. Disagreeing with his conclusions, Ludwig and Thiry (108) completely denervated the heart, and still obtained an increase in the blood pressure and in the heart-rate. They concluded that the stimulation of the cord caused an increase in blood pressure via splanchnic vasoconstriction, and this increased blood pressure directly speeded up the heart.

The brothers Cyon (38-42) took issue with von Bezold and Ludwig and Thiry, and demonstrated that both phenomena were inde-

pendent, and could be made to appear separately. Following up this line of reasoning, they directly stimulated the nerves given off to the heart from the stellate ganglion, and discovered the accelerator nerves to the heart. The conflict between these experiments, in which an increase in blood-pressure was accompanied by an augmented heart-rate, and Marey's experiments, was quickly recognized by Pokrowsky (125), and soon after Bernstein (10), Asp (2), and Knoll (188, 89) determined that the difference was to be found in the state of the vagus nerves. Only when these nerves were intact did slowing regularly follow increase in blood pressure, while if they were sectioned, variable results were obtained.

Without exception these authors suggested that the pressure of the blood in the cerebral arteries had a direct effect upon the vagal centres in the medulla (see also 23, 102). At least a partial solution had already been afforded, of which they seem to have been unaware. The year before (1866) Cyon and Ludwig (40) had reported the discovery of what they called the depressor nerve. This nerve they believed to originate in the heart, and was clearly not a motor nerve, for stimulation of the distal portion of the cut nerve was entirely without effect. When, however, the central end was stimulated, the blood pressure fell, and the heart beat slowed. Since section of the vagus suppressed the slowing, but had no effect on the blood pressure, Cyon and Ludwig decided that the response consisted of two parts; a reflex stimulation of the vagus, and a reflex inhibition of the vasomotor centre. Their discovery was rapidly confirmed by Kowalewsky and Adamük (99).

They did not, however, attempt to explain the results of Marey's experiments on the basis of stimulation of depressor endings, this was suggested somewhat later by Cyon (41, 42), who believed at that time that slowing only infrequently occurred with increased pressure. The final elucidation of the rôle of the depressor nerve must be credited to Sewall and Steiner, who showed that the depressor was stimulated normally by increasing arterial pressure (145). Similar experiments were performed at nearly the same time by Konow and Stenbeck (96), who came to similar conclusions. Later, Bayliss (4) and Brücke (28, 29) demonstrated that the depressor nerve inhibited the sympathetic centre at the same time that it ex-

cited the vagus centre, and thus explained the slight slowing occasionally noted even when the vagi were both cut (see also 83). Finally, Köster and Czermak (97, 98) demonstrated both anatomically and physiologically that the depressor nerve originated in the arch of the aorta, and not, as Cyon and Ludwig and all subsequent investigators supposed, in the heart itself.

Even before the function of the depressor nerves was recognized the existence of another pressure regulating system was suspected, and the evidence for it grew along with development of the knowledge of the depressor nerve. Asp, Bernstein, and Knoll, who probably had not learned of Cyon's and Ludwig's discovery, attributed the vagal discharge evoked by increasing intra-arterial pressure to a direct effect of the increasing blood pressure upon the vagal centre in the medulla. In doing this they were but adopting suggestions that had already been made (117, 119, 120).

Astley Cooper (36) and Magendie (109) had some years before made a study of ligation of the common carotid artery, both in man and experimental animals, and it rapidly became established that this procedure produced both an increase in blood pressure and an increase in heart-rate. A number of investigations then suggested that the vagal and vasomotor centres in the medulla were directly influenced by the pressure of blood in the brain, which controlled the blood flow through that organ (81). This mechanism, which served to protect the brain from a deficient or excessive blood supply reacted to a diminished supply by increasing the blood pressure and increasing the rate of the heart, and to an excessive flow by decreasing the blood pressure and the heart-rate. To demonstrate this, Francois-Franck injected fluid centrally into the brain via the carotid arteries, and obtained exactly what he sought; a decreased rate of heart beat with increasing intracranial pressure (56-58). His experiments were confirmed by Eyster and Hooker (51, 52) after several workers had failed to obtain clear-cut effects (94). Thus two pressure regulating systems were recognized, one the reflex regulation through the depressor nerve, and the other the direct influence of intracranial blood pressure on the vasomotor and cardiac centres.

There were some, however, to whom this explanation did not seem sufficient. Latschenberger and Deahna wrote that the blood pressure

is reflexly regulated through afferent fibres from the heart and all the blood vessels, which respond to changes in blood pressure (103). These are in constant action, and the blood pressure is maintained through the coördinated activity of all of them. They offered no experimental proof of this view, but soon after Heger (67), and later Spallita and Consiglia presented evidence that indicated that the internal coats of arteries are sensitive, and Heger suggested that these endothelial sense organs might act to regulate blood pressure. Extending Heger's observation, Pagano (123) found that injections of solutions of nicotine, formalin, chloral, etc., into the common carotid produced slowing of the heart even when the internal carotid was ligated. When the sympathetic chain of that side was destroyed, slowing did not appear. This slowing he termed an indirect effect, preferring not to designate it as reflex in the absence of vigorous proof. The vascular surface from which this "indirect" slowing could be elicited was situated in the common carotid from its origin to its bifurcation. "I might even add," he reported, "that in all probability the most sensitive region is that nearest the bifurcation of the carotid."

Almost simultaneously Siciliano came to similar conclusions (146). Analysing the response to carotid closure, he found that the response consisted of two phases, an immediate and a delayed, of which the latter alone could be attributed to cerebral anemia. The first phase could not be produced to any marked degree by occlusion of the vertebrals, but only by closure of the carotids. What is more, closure of the internal carotids was ineffective, while occlusion of the common carotids produced marked changes. When the sympathetics in the neck were destroyed the phenomenon could not be obtained. These phenomena he considered to be due to a complex nervous reaction, having much similarity to a reflex. "A final point requiring explanation," he wrote, "is to know how the brusque decrease in pressure in the common carotid can give rise to a decrease, or even a temporary suspension of the tonicity of the vagus nerves along with certain excitation phenomena. *The simplest concept of the entire phenomenon is to consider the pressure normally exerted upon the walls of the carotids as one of the factors that tends to maintain the tonic activity of the cardiac inhibitory nerves.*"

While few workers accepted these experiments (85, 86, 148), the suggestions made by Pagano and Siciliano did receive some favorable attention, especially by Hofman (80). It remained for Hering, however, whose teacher Knoll (90) had noticed the depressor effects obtainable by stimulation of the central end of the glossopharyngeal nerve, to demonstrate that the carotid sinus contains an important collection of sense organs which respond to the blood pressure and, with the similar sense organs in the arch of the aorta, regulate heart-rate and blood pressure (70-77, 91-93). Finally, only recently, the work of Braeucker and Bronk has shown that, while the arch of the aorta and the carotid sinuses contain the greatest collection of pressoreceptors, much of the rest of the arterial tree is also liberally supplied.

The discovery of the function of reflexes from the carotid sinus, and the recognition of their presence in man, affords a final explanation of the cardiac inhibition obtained in man by pressure over the region of the carotids. Waller was probably the first to notice this (161-164). He had become interested in mechanical stimulation of nerves in man by mechanical pressure, and had already recorded the results of stimulation of the ulnar nerve. Attempting next to stimulate the vagus or sympathetics in the neck, he applied pressure in the neck just behind the ramus of the lower jaw. He noted first a slight increase, followed by a decrease in the rate of the heart, terminated, if pressure were maintained, by syncope. The experiment is usually, however, credited to Czermak, who six years later produced much more definite slowing by pressure applied over the right carotid, at the point where there was a small, pulsating swelling. He also believed that he had directly and mechanically stimulated his vagus nerve (43, 44).

Soon after Quincke (134) found that the procedure was successful in a significant proportion of all subjects, both in health and disease, and Bensen introduced it with success as a method for arresting attacks of paroxysmal tachycardia (6). While attempting the experiment with one of his students, Tannhoffer was astounded to notice the subject collapse, and remain unconscious for some seconds (152). Similar syncope was observed by Hirsch and Stadler (78), and by Heinecke, Müller and Hösslein (69) and has only recently

been carefully studied by Weiss (175). Hering having shown that Czermak's experiment was in fact due to mechanical stimulation of the carotid sinus, Weiss was able to demonstrate that in some individuals the carotid sinus reflex is particularly sensitive, so that a slight pressure may evoke an extraordinarily strong vagal discharge.

Summary

The neurogenic theory forms the background for the history of the vagus nerve from Haller to Hering. It not only led to the discovery by the Webers of the effect on the heart of stimulation of the vagus, but upon it was largely based the theory of vagal inhibition developed by the Webers.

While the discovery of vagal inhibition by the Webers undoubtedly constitutes the outstanding contribution to the development of the subject, an appreciation of the function of the nerve in the intact individual was developed largely through the study of the reflexes which evoke vagus discharge. Of these, the most important are those in response to changes in blood pressure within the aorta and the carotid sinus.

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JEWISH AND CHRISTIAN INTERMARRIAGES IN BUDAPEST: A FOOTNOTE TO RECENT SOCIAL HISTORY[1]

RAYMOND PEARL
AND
CELESTE FRANKLIN

I

Many observers of the changing social scene have been impressed with the plain fact that the enormous improvements in the mechanisms of transportation and communication that began with the effective application of steam power in the early nineteenth century, and have since progressed with then undreamed of acceleration to the almost incredible end results of the present day, have had a profound biological effect upon humankind in the aggregate. This biological effect, which is literally world-wide in its scope, and increases in magnitude all the time, is to *diminish* steadily by interbreeding the innate biological differences that formerly existed between races and other differentiated groups of human beings. Statisticians have long known that propinquity is a major factor in determining who shall marry whom, or indulge in extra-marital reproducing. Because it is so easy to get about nowadays, by train or bus or ship or automobile or airplane, propinquity relations formerly rather sharply limited by the difficulties of living our lives very far removed from the places of our birth have now taken on an areal comprehensiveness and fluidity that leaves no one surprised to find anybody married to anybody else conceivable.

Commenting upon another aspect of the fact that the world is literally becoming "a small place" Oliver[2] says (p. 51), in one of the most remarkable treatises on politics ever written:

[1] From the Department of Biology of the School of Hygiene and Public Health, with greetings to Arnold Klebs, as a token of appreciation of his keen interest in, and deep understanding of, the social aspects of human biology.

[2] Oliver, F. S. The Endless Adventure. Personalities and Practical Politics in Eighteenth-Century England. Two Vols. in one. *Boston* and *New York* (Houghton Mifflin Co.) 1931. Pp. xvii + 428 + 326.

During the lifetime of many of us the world has shrivelled and puckered like a child's balloon slowly deflating, so that now we find ourselves cheek-by-jowl with peoples and regions that fifty years ago were regarded as half mythical. And not only do we now see strange faces at close quarters, but we are beginning to have confidence that some day we may be able to read the hearts that belong to them.

Pearl [8] (pp. 267-8) in discussing still another aspect of the same matter, namely the future of war as phenomenon and as social agent, said:

Is there any reason to suppose that this biologically natural process, with its characteristic of almost rhythmic recurrence, will ever come to an end? It seems to me there can be such a hope only in the long—very, very long—run. And the only reason I can see for even this deferred hope is the already great and rapidly increasing ease, speed, and cheapness of transportation and communication between all parts of the world. The slow but steady and sure biological effect of easy getting about will inevitably be more and more interbreeding, with a gradual lessening of the racial and national differences between human beings. In the far-off end all mankind will presumably be a rather uniform lot; all looking, thinking, and acting pretty much the same way, like sheep. National or racial isolation has even now become extremely difficult to maintain; indeed in a quite literal sense the attempt to maintain such isolations already threatens group survival in not a few instances. In the long run they cannot and will not be maintained. Just in proportion as they diminish so will the frequency of wars diminish. But the diminution seems likely to be at a fearfully slow rate; it will be a long time yet before the *last* war is fought. And a low cynic might suggest that even war, horrid and stupid as it is, would be preferable to that deadly uniformity among men towards which we are slowly but surely breeding our way.

While ease of transportation and communication is a major factor in promoting intermarriage between members of differentiated groups, other social forces are also involved, such as slow but steady weakening of religious and other tabus that, even in the most cultured and civilized societies, have long operated with greater or less force to keep human birds of a feather flocking together. Many things in present-day sociality combine to diminish the force of the old marriage tabus. Migration away from the group of origin is potent in freeing the individual from the old inhibitions. So also has been the wide dissemination of the basic ideologies of natural

[8] Pearl, R. Biology and human trends. *Jour. Washington Acad. Sci.*, Vol. 25, pp. 253-272, 1935.

science, on the one hand, and of militant socialism, on the other hand. Young people today, and especially urban dwellers, not only feel but actually are enormously freer to marry whomever they please, without fear or even thought of unpleasant social consequences.

There is general agreement among persons competent to judge that the mixture of racially and otherwise differentiated human groups by interbreeding is proceeding at an accelerated pace at the present time. But there is extremely little to be found in the literature in the way of precise statistical observations by which this conclusion based upon general experience and inference can be proved and quantified. For that reason it seems worth while to publish the present brief discussion of the marriages between Christians and Jews in Budapest in the period between 1897 and 1935 inclusive. A certain measure of particular interest attaches to this intermixture because of the march of events in Central Europe since World War I, and the accompanying phenomena of individual and social psychology in those regions with the ugly growth of anti-Semitism.

II

The basic data for this study are derived from successive volumes of that model of municipal statistics, currently prepared under the direction of Dr. Illyefalvi I. Lajos, the *Budapest Székesfőváros Statisstikai és Közigazgatási Evkönyve*[4] (Statistical-Administrative Yearbook of the City of Budapest). The published data on marriages in these volumes are unusually detailed, and are tabulated in such a manner that by relatively little rearrangement it became possible to form the derivative tables here presented. Unfortunately, as will be seen from Tables 1 and 2, there are some gaps in the data as here given. These are due to two causes: first, the omission of certain of the marriage tables in certain years in the original source, and especially during the years of World War I; second, our inability to locate an absolutely complete file of the Yearbook in any of the large American libraries that we were able to consult. However, as will presently appear, these gaps in the record are not of serious moment in forming a judgment as to the general trend during the 39 year period.

[4] Hereinafter referred to for convenience as BSSKE.

Table 1 shows the general statistical course of Christian × Christian, Jew × Jew, and Christian × Jew marriages in Budapest, in both absolute and relative figures, and also the general marriage rate (per 1000 of population).

TABLE 1

CHRISTIAN AND JEWISH MARRIAGES IN BUDAPEST, 1897-1935

Year	Marriages involving one or two Christians	Marriages involving one or two Jews	Marriages involving a Christian and a Jew	Percentage of mixed marriage to All involving a Christian	Percentage of mixed marriage to All involving a Jew	General marriage rate per 1000 pop.[1]
1897	4746	1402	138	2.91	9.84	9.6
1898	4988	1383	145	2.91	10.48	9.6
1897-8	9734	2785	283	2.91	10.16	9.6[2]
1902	5138	1509	236	4.59	15.64	8.6
1903	5137	1418	209	4.07	14.74	8.4
1904	5722	1576	217	3.79	13.77	9.2
1905	5808	1560	224	3.86	14.36	9.2
1906	6075	1745	267	4.40	15.30	9.6
1902-6	27880	7808	1153	4.14	14.77	9.0[2]
1908	6734	1897	333	4.94	17.55	10.0
1909	6570	1884	334	5.08	17.73	9.6
1910	6681	1910	335	5.01	17.54	9.5
1911	7184	2013	384	5.34	19.08	9.9
1912	7527	2151	411	5.46	19.11	10.2
1908-12	34696	9855	1797	5.18	18.23	9.84[2]
1920	9300	3033	672	7.23	22.16	12.2
1921	8743	2664	614	7.02	23.05	11.6
1922	9350	2789	706	7.55	25.31	12.2
1923	8894	2755	711	7.99	25.81	11.6
1924	8027	2454	616	7.67	25.10	10.4
1925	8048	2305	652	8.10	28.29	10.1
1926	8247	2260	658	7.98	29.12	10.2
1927	8771	2430	684	7.80	28.15	10.8
1928	9077	2462	665	7.33	27.01	11.1
1929	8832	2240	628	7.11	28.04	10.6
1920-29	87289	25392	6606	7.57	26.02	11.08[2]
1930	9066	2404	714	7.88	29.70	10.9
1931	8878	2331	708	7.97	30.37	10.4
1932	9040	2262	713	7.89	31.52	10.4
1933	8985	2364	705	7.85	29.82	10.4
1934	9902	2520	767	7.75	30.44	11.2
1935	10048	2562	762	7.58	29.74	11.3
1930-35	55919	14443	4369	7.81	30.25	10.77[2]

[1] The general marriage rate (per 1000 of population) was as follows for years not given in the table:

1899 — 9.4 1901 — 8.7 1918 — 9.3
1900 — 8.8 1907 — 10.1 1919 — 24.0

[2] Unweighted averages.

From certain of the data of Table 1 there has been plotted Fig. 1, on an arithlog grid, to show graphically the trends of mixed Christian × Jewish marriages, as percentage of all Christian and of all

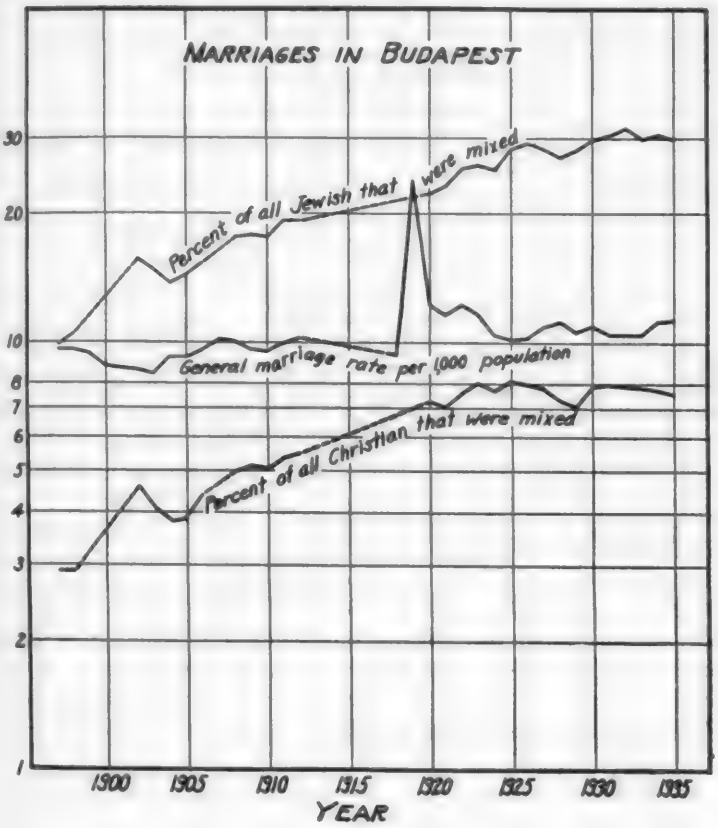

Fig. 1.

Trends of mixed Christian × Jewish marriages in Budapest, 1897-1935 inclusive, and of the general marriage rate. Solid portions of the trend lines represent observations. Where the lines are broken observations were not available. Arithlog grid.

Jewish marriages, and the relation of these trends to that of the general marriage rate.

From Table 1 and Fig. 1 the following points may be noted:

1. In the 16 years from 1919-35 the general marriage rate in Budapest was higher on the whole, by small but definite amounts, than it had been in the prewar years 1897-1912 inclusive. The sharp, and relatively enormous, rise in the general marriage rate in 1919 immediately following the end of the war is noteworthy.

2. The proportion of mixed Christian × Jewish marriages to all marriages of either Christians or Jews increased generally during the period, and to a marked degree. Thus the percentage of mixed marriages to all Christian marriages in 1935 was 2.6 *times* greater than in 1897. The percentage of mixed marriages to all Jewish marriages was 3.02 times greater in 1935 than in 1897. If instead of terminal single years we compare the weighted averages of the first two years of the period covered (1897-8) with those of the last six years (1930-5) it is seen that the mixed: Christian percentage was 2.7 times greater in the latter than in the former period, and the mixed: Jewish percentage was 2.98 times greater. There can be no possibility of doubt that in the city of Budapest the twentieth century has witnessed a very substantial increase in the relative extent of maritally legitimized interbreeding between Christians and Jews, two clearly differentiated groups of human beings.

3. The levels of the two trend lines are different as a consequence of the composition of the population. Just prior to the beginning of the present century just under 3 out of every 100 Christians who married in Budapest married Jews. At the same period approximately 10 out of every 100 Jews who married in the same city took Christians as their spouses. In 1935 these proportions had become respectively about 8 instead of 3, and 30 instead of 10. When nearly one-third of a differentiated human group is marrying outside its group the original differentials, of every sort including anthropological, psychological, and social, are plainly being melted away at a rapid rate.

4. Up to the year 1925 the percentage trends of mixed to " pure " marriages followed generally parallel courses at their respective relative levels. But after 1925 they took different paths. How different can best be shown by comparison of the equations of the straight

lines fitted by the method of least squares to the two sets of observations from 1925 to and including 1935. These equations are:

$$y_1 = 7.825 - .0126x, \text{ and} \quad \text{(i)}$$
$$y_2 = 27.715 + .263x, \quad \text{(ii)}$$

where y_1 denotes the percentage that mixed Christian × Jewish marriages are of all Christian marriages; y_2 the percentages that the mixed marriages are of all Jewish marriages; and x years after 1924. It will be noted that the slope constant of the first equation is minus in sign, while that of the second equation is plus, and is in numerical magnitude more than 20 times greater. So then it appears that since 1925 the proportion of mixed Christian × Jewish marriages to all Jewish marriages has substantially increased, while the proportion of these mixed marriages to all Christian marriages has shown a slightly declining trend.

Since the number of mixed marriages in each year necessarily remains the same whether it is referred to total Christian or to total Jewish marriages as a base for relative or percentage expression it is evident that the failure of parallelism in the percentage trend lines since 1925 must be connected with the differential changes in the movements of the respective populations at risk of marrying. The key to the picture is found in the comparative growth of the Jewish and Christian portions of the population. The following figures show the absolute populations as of the stated dates (computed from data in Table 49, p. 78, BSSKE 24, 1936):

	Year					
Group	1890	1900	1910	1920	1930	1935
Jewish	104,290	168,985	203,687	215,512	204,371	201,069
Christian	400,471	562,439	675,237	711,984	800,182	844,592

From these figures it is evident that while the total Christian population increased from 1890 to 1935 by the combined processes of natural growth and net immigration, without any break in the upward trend, the same was true for the total Jewish population only up to and including the 1920 count—that is to say until shortly after the end of World War I. Thereafter a decline was registered. That decline amounted to —5.2 percent for the period 1921-1930,

TABLE 2

BRIDEGROOMS AND BRIDES IN MARRIAGES IN BUDAPEST, 1897-1935

Year	Total Christian bridegrooms	Percent of these whose brides were Christian	Percent of these whose brides were Jewish	Total Jewish bridegrooms	Percent of these whose brides were Christian	Percent of these whose brides were Jewish	Total Christian brides	Percent of these whose bridegrooms were Christian	Percent of these whose bridegrooms were Jewish	Total Jewish brides	Percent of these whose bridegrooms were Christian	Percent of these whose bridegrooms were Jewish
1897	4680	98.5	1.5	1330	5.0	95.0	4674	98.6	1.4	1336	5.4	94.6
1898	4908	98.7	1.3	1318	6.1	93.9	4923	98.4	1.6	1303	5.0	95.0
1897-8	9588	98.57	1.43 ± .09	2648	5.51 ± .30	94.49	9597	98.48	1.52 ± .08	2639	5.19 ± .29	94.81
1902	5009	97.9	2.1	1402	9.2	90.8	5031	97.4	2.6	1380	7.8	92.2
1903	5027	98.0	2.0	1319	8.3	91.7	5038	97.8	2.2	1308	7.6	92.4
1904	5606	98.2	1.8	1475	7.9	92.1	5621	97.9	2.1	1460	6.9	93.1
1905	5695	98.1	1.9	1449	7.8	92.2	5697	98.0	2.0	1447	7.7	92.3
1906	5920	98.1	1.9	1633	9.5	90.5	5963	97.4	2.6	1590	7.0	93.0
1902-6	27257	98.06	1.94 ± .06	7278	8.57 ± .22	91.43	27350	97.72	2.28 ± .06	7185	7.38 ± .21	92.62
1908	6559	97.6	2.4	1739	10.1	89.9	6576	97.3	2.7	1722	9.2	90.8
1909	6397	97.5	2.5	1723	10.0	90.0	6409	97.3	2.7	1711	9.4	90.6
1910	6499	97.6	2.4	1757	10.4	89.6	6528	97.2	2.8	1728	8.9	91.1
1911	6989	97.3	2.7	1824	10.7	89.3	6995	97.2	2.8	1818	10.4	89.6
1912	7299	97.5	2.5	1968	11.6	88.4	7344	96.9	3.1	1923	9.5	90.5
1908-12	33743	97.50	2.50 ± .06	9011	10.58 ± .22	89.42	33852	97.18	2.82 ± .06	8902	9.48 ± .21	90.52
1920	8886	97.1	2.9	2775	14.9	85.1	9042	95.4	4.6	2619	9.9	90.1
1921	8387	96.9	3.1	2406	14.8	85.2	8485	95.8	4.2	2308	11.2	88.8
1922	8966	96.4	3.6	2467	15.6	84.4	9028	95.7	4.3	2405	13.4	86.6
1923	8522	96.0	4.0	2416	15.4	84.6	8555	95.7	4.3	2383	14.2	85.8
1924	7711	96.1	3.9	2154	14.7	85.3	7727	95.9	4.1	2138	14.0	86.0
1925	7690	96.2	3.8	2011	17.8	82.2	7754	95.4	4.6	1947	15.1	84.9
1926	7878	96.3	3.7	1971	18.7	81.3	7958	95.4	4.6	1891	15.3	84.7
1927	8422	96.0	4.0	2095	16.7	83.3	8436	95.9	4.1	2081	16.1	83.9
1928	8709	96.6	3.4	2165	17.0	83.0	8780	95.8	4.2	2094	14.2	85.8
1929	8514	96.4	3.6	1930	16.5	83.5	8522	96.3	3.7	1922	16.1	83.9
1920-29	83685	96.41	3.59 ± .04	22390	16.10 ± .17	83.90	84287	95.72	4.28 ± .05	21788	13.78 ± .16	86.22
1930	8692	96.1	3.9	2064	18.1	81.9	8726	95.7	4.3	2030	16.7	83.3
1931	8485	96.3	3.7	2016	19.5	80.5	8563	95.4	4.6	1938	16.3	83.7
1932	8663	96.1	3.9	1926	19.6	80.4	8704	95.7	4.3	1885	17.8	82.2
1933	8606	96.2	3.8	2038	18.6	81.4	8659	95.6	4.4	1985	16.4	83.6
1934	9465	96.5	3.5	2190	20.0	80.0	9572	95.4	4.6	2083	15.8	84.2
1935	9632	96.4	3.6	2216	18.8	81.2	9702	95.7	4.3	2146	16.1	83.9
1930-35	53543	96.28	3.72 ± .06	12450	19.08 ± .24	80.92	53926	95.59	4.41 ± .06	12067	16.52 ± .23	83.48

and of —1.6 percent for the period 1931-35. So then it becomes clear that, in the period from 1920 on to the end of the record here discussed, (a) the increase in the absolute number of mixed Christian × Jewish marriages shown in Table 1, when referred to all Jewish marriages as a base in a Jewish population where the total number was declining must necessarily lead to a steadily rising percentage trend line on that base as is shown graphically by the top line of Fig. 1. On the other hand, (b), the Christian population increased between 1920 and 1935 by 18.6 percent of the 1920 figure, while the increase in the number of mixed marriages from 672 of 1920 to the 762 of 1935 was only 90, or 13.4 percent of the 1920 figure. So a flattening of the relative trend line based on all Christian marriages was inevitable.

III

Table 2 presents the data for brides and bridegrooms separately in Budapest marriages. From this table some insight may be gained regarding some perplexing questions about the psychology of human mate choosing.

The first point made clear by Table 2 is that, in the Budapest experience covered, Christian brides were more apt to marry Jewish men than Christian bridegrooms were to marry Jewish women. The differences are small but regular, as appears in the following summary of the amounts by which the percentage of Christian brides marrying Jewish men exceed the corresponding percentages for Christian bridegrooms making mixed marriages.

Period	Percent of Christian brides marrying Jewish men	Percent of Christian bridegrooms marrying Jewish women	Difference
1897-8	1.52	1.43	0.09 ± .12
1902-6	2.28	1.94	.34 ± .09
1908-12	2.82	2.50	.30 ± .09
1920-9	4.28	3.59	.69 ± .06
1930-5	4.41	3.72	.69 ± .09

Plainly since the beginning of this century there has been a significantly higher proportion of mixed marriages in Budapest of the

type Jewish ♂ × Christian ♀ than of the opposite type Christian ♂ × Jewish ♀. Furthermore the differential between the two types has plainly increased with time, and particularly since the end of World War I.

If the matter be approached in the first instance from the standpoint of the Jews in Budapest the figures indicate, as would be expected, that Jewish women were less apt to marry Christian men, than Jewish men were to marry Christian women. The magnitude of this differential tendency is shown by the following system of differences:

Period	Percent of Jewish bridegrooms marrying Christian women	Percent of Jewish brides marrying Christian men	Difference
1897-8	5.51	5.19	0.32 ± .42
1902-6	8.57	7.38	1.19 ± .30
1908-12	10.58	9.48	1.10 ± .30
1920-9	16.10	13.78	2.32 ± .23
1930-5	19.08	16.52	2.56 ± .33

Altogether it seems a reasonable inference that in Budapest the mutual attractiveness leading persons to embark upon a mixed marriage—a step that is definitely, even if not greatly, outside the normal folkways in western civilizations—is definitely greater between Jewish males and Christian females than between Christian males and Jewish females. The purely statistical data at hand are not sufficient to determine in detail what lies behind and is the cause of this differential in preference between the two sexes in the contraction of mixed marriage. Conceivably economic factors may play a significant rôle in it—perhaps wealth may have contributed to the making of Jewish men more attractive as grooms to Christian women than Jewish brides were to Christian men. But in any case the motivations were certainly more complicated than got recorded in official statistics.

But even more remarkable than this differential itself in the mixed marriage is the fact that it has increased in magnitude since the war. This is a social phenomenon extremely difficult to explain. The fact that since the end of World War I Germany has been the focal

point of a bitter and ever-growing wave of anti-Semitism is so undoubted and so undeniable as to need no elaborate documentation. Particularly has this deplorable piece of social psychology become outspokenly vicious with the achievement of complete power of the Nazi regime during the 1930's. Now, to be sure, Budapest is not Germany, or even in Germany. Yet anyone who would seriously allege that the anti-Semitic ideology and propaganda of the German Reich in recent years has not infected *in some degree* neighboring peoples in Europe must surely be regarded as something less than an acute or completely realistic social observer. The spectacle of great ships loaded with dispossessed Jewish refugees cruising about the world's oceans unable to discharge their pitiful cargoes anywhere, indicated sufficiently that the normally to be expected warmhearted sympathy for human beings in distress has been everywhere replaced, to some extent at least, by cautious and questioning reservations. The voluble expressions of verbal sympathy coupled with polite but very firm regrets that *their* several countries found themselves quite unable to receive Jewish refugees from Germany and Austria, which so unanimously filled the *procès verbaux* of the meetings of the international delegates at Evian were eloquent testimony to the infective power of a whispering campaign that shouted instead of whispering.

Suppose now for a moment we attempt to translate this group psychology down to the level of the individual, and particularly the individual about to marry. Everywhere in the western world, long before Nazism was ever heard of, mixed marriages between Christians and Jews were less lightly entered upon than those between Christians and Christians or those between Jews and Jews. Always it has taken a bit more courage to embark upon a mixed marriage. More thought was taken about the matter, and for an obvious reason. Certain social disabilities and discomforts were entailed. Normally these entails were not terribly serious—mutual love could fairly easily overcome them. But they were always there. It is probably safe to say that even before 1920 not one in a million mixed Jewish × Christian marriage was consummated without some doubt or hesitation over the social consequences. If this were so in normal times, is it not reasonable to suppose that the searchings of

heart and takings of thought are much greater today, and have been at any time during the last five years, on the part of any person in Europe, or almost anywhere else in the world for that matter, on the part of persons impelled to contract a mixed marriage involving a Jew?

It is in the face of these considerations that the trends of mixed marriages in Budapest since World War I seem so remarkable and difficult completely to explain. Data are lacking, as has been said, for a complete analysis. But a description of the phenomenon, in quantitative terms seems worth putting on record.

Fig. 1.

EDWIN KLEBS

1834-1913.

UNIV. OF MICH.

UEBER DIPHTHERIE*

EDWIN KLEBS

M. H.! Ich habe selten eine so grosse Befriedigung gefühlt als gestern in dem Momente, als sich mir bei meiner ersten und einzigen Unterredung über diesen Gegenstand mit dem Herrn Referenten sofort die Ueberzeugung aufdrängte, dass ich, vom anatomisch-pathologischen Standpunkte ausgehend, ganz unabhängig von den Klinikern, in dieser Beziehung zu genau denselben Resultaten gekommen bin; ich habe nämlich allmählich im Laufe der Jahre gleichfalls die Ueberzeugung gewonnen, dass dasjenige, was man als Diphtherie bezeichnet, ein Mannigfaches ist; und ausserdem dass nicht, wie es früher üblich war, unter den Begriff der Diphtherie alle jene Fälle zu beziehen sind, in denen überhaupt ein fibrinöses Exsudat in Gestalt von Membranen an der Oberfläche der Schleimhaut sich bildet. Zunächst muss in dieser Frage unzweifelhaft der klinische Standpunct, d. h. der Verlauf des betreffenden Prozesses massgebend sein; dieser aber nöthigt uns, Fälle von Membranbildung, welche nicht den eigenthümlichen infectiösen Character der Diphtherie an sich tragen, von diesem Begriffe auszuschliessen. Ich will indess nicht unterlassen, auch einzelne von diesen Fällen kurz zu berühren und an Zustände erinnern, in denen wir ohne Weiteres von Diphtherie des Darmes sprachen, wenn wir Dysenterie meinten, von Diphtherie der Harnblase, wenn faulige Blasencatarrhe zu Necrose der Schleimhaut dieses Organs führten; ich will ferner daran erinnern, wie jede Membranbildung als diphtherisch bezeichnet wurde, auch wenn sie überhaupt nicht den Character der Infectiosität an sich trug, wie z. B. die chronische, recidivirende croupöse Affection der Lunge, von der ich, da Sie Alle diese Affection wohl selten im microscopischen Bilde gesehen haben, ein Präparat mitgebracht habe. Um das ganz kurz zu bemerken, will ich hier sagen, dass es sich da um einen ganz differenten Prozess handelt, in dem die fibrinösen Ablagerungen nicht an der Oberfläche der Schleimhaut auftreten, sondern

* Reprinted from *Verhandlungen des Congresses für Innere Medicin.* Zweiter Congress gehalten zu Wiesbaden, 18.-23. April 1883. Wiesbaden, Verlag von J. F. Bergmann, 1883, pp. 139-154.

als lockere Fibrinfäden, die ganz allmählich durch neue Lagen sich verdicken und verlängern zu den grossen bronchialen Gerinnseln, welche ausgehustet werden. Lassen wir also, wozu wir, glaube ich, berechtigt sind, einmal vollständig die Fälle einfacher faserstoffiger Exsudation aus der Betrachtung, und beschränken uns nur auf die infectiösen Formen, in denen sich progressiv entwickelnde Faserstoffgerinnsel infectiösen Characters an der Oberfläche der Schleimhaut bilden. Wenn wir diese Fälle ausschliesslich in Betracht ziehen, so geht schon aus der Multiplicität der Angaben bezüglich der morphologischen Zusammensetzung, wie sie Herr College Gerhardt hervorhob, hervor, dass es sich um etwas Verschiedenartiges auch in dieser Gruppe von Erkrankungen handeln muss, und ebenso treten uns grosse Verschiedenheiten im Verlaufe entgegen, wie der Herr Vorredner dieses gleichfalls im Einzelnen geschildert hat. Ich muss mich darauf beschränken, die Angaben bezüglich des Vorkommens von Microorganismen in diesen Fällen zu durchmustern und meine eigenen Erfahrungen darüber in Kürze mitzutheilen.

Wenn wir zunächst historisch vorgehen, so sehen wir, dass die ersten genaueren Angaben über das Vorkommen von Schizophyten in den Membranen nicht weiter zurückzuführen sind als auf Oertel. Ich erinnere daran, dass Tommasi und Hüter sich wesentlich beschränkten auf den Nachweis der Anwesenheit von Monaden im Blute. Also Oertel ist der erste, der eingehende Beobachtungen lieferte von den Organismen, die in den Faserstoff-Membranen vorkommen; er bezeichnete dieselben als Micrococcen, die in grosser Menge sowohl in den Membranen vorhanden sind, als auch von diesen aus in die Schleimhaut eindringen und sich in den übrigen Organen des Körpers verbreiten. Dieselben Befunde sind später von Letzerich bestätigt, der ebenso, wie schon früher Oertel, durch diese Befunde zu Untersuchungen über die Möglichkeit der Uebertragung der Diphtherie durch diese Organismen angeregt wurde. Der dritte, der in dieser Weise vorging, war bekanntlich Eberth, dessen Befunde gleichfalls die Anwesenheit grosser Mengen von Micrococcen in der Diphtherie-Membran bestätigten, welche sich durch die gelbliche Färbung auszeichneten, die namentlich nach Uebertragung auf die Cornea in sehr auffälliger Weise hervortrat. Ich habe später dann selbst eingehendere Untersuchungen über diese Microorganismen gemacht, Reinculturen derselben ausgeführt und

konnte durch einen meiner Schüler, Graham Browne, gleichfalls nachweisen, dass es keine Schwierigkeiten hat, diese Microorganismen auf die Cornea zu übertragen. Es schien demnach gerechtfertigt, dieselben als die eigentliche Ursache der Diphtherie zu betrachten, allein bei weiteren Untersuchungen geeigneter Fälle stellten sich mir verschiedenartige Befunde heraus, welche es mir zweifelhaft machten, ob überhaupt ein und derselbe Organismus in allen Fällen von Diphtherie in Betracht kommen könne. Ich habe in dem von mir und meinen Freunden redigirten Archive für exp. Path. und Pharm. diese Beobachtungen niedergelegt, die ich Gelegenheit hatte in Prag zu machen, wo ich zuerst jene foudroyanten Fälle von Diphtherie kennen gelernt hatte, die mir bis dahin unbekannt waren. Es sind dieses jene Formen der Diphtherie, bei denen, wie dieses schon Buhl und Oertel beschrieben hatten, bei geringer Rachenaffection frühzeitig schwere Allgemeinerscheinungen mit vorwiegend nervösem Character auftreten, die sich anatomisch characterisiren durch ausgedehnte haemorrhagische Bildungen an der Oberfläche des Gehirnes oder des Rückenmarkes. In einem solchen Falle war ich in der Lage einen Befund zu constatiren, den ich nicht als gleichartig mit den früher gewonnenen Befunden betrachten konnte, weil es sich um einen vollkommen intacten Fall handelte, der in wenigen Tagen zum Tode geführt hatte, bei dem keine Application eines Mittels auf die kleine Krankheitsfläche ausgeführt worden war, und wo sich nun die von Oertel und Buhl beschriebenen Befunde in exquisitester Weise vorfanden. Ich habe damals angegeben und kann noch Präparate davon vorlegen, dass in diesem, wie in einer Reihe ähnlicher Fälle, die ich in den folgenden Jahren beobachtete, und welche zum Theil von Eppinger und von anderen meiner damaligen Assistenten beschrieben sind, an der Oberfläche der Faserstoff-Membranen eine continuirliche Schicht ziemlich langer, gerade stehender oder etwas gebogener Fäden sich vorfanden, welche ich damals mit einem Aehrenfelde verglich. Es fanden sich an der Oberfläche noch Reste der Plattenepithelien vor und erst unterhalb dieser Stäbchenschicht beginnt dann die gewöhnliche Faserstoffmembran, die eine bedeutende Dicke haben kann und bei diesen frischen Fällen ausserordentlich wenige Zellen enthält. Daran schliesst sich die glatte Schleimhautschicht, welche reichlicher von zelligen Elementen durchsetzt ist. Ein zweites fremdes Element in der Fibrinschicht sind nun kugel-

förmige Körper: Ballen, welche aus sehr kleinen und ausserordentlich dicht gedrängten Micrococcen bestehen. Auch für diese letztere Form kann ich noch wohlerhaltene Präparate vorlegen. Es liess sich demnach die so häufig constatirte Anwesenheit von Micrococcen auch in diesen Formen festellen, aber ebenso sicher gelang der Nachweis, dass unter Umständen fadenförmige Bildungen in den Membranen vorkommen können. Es wäre der Einwand zu machen und ist gemacht worden, dass es sich hier möglicher Weise nur um zufällige Verunreinigungen gehandelt habe, allein es zeigte sich alsbald, dass diese selbe Form von ziemlich langen fadenförmigen Bildungen in enormer Menge an den Blutgefässen des Gehirnes vorhanden waren. Dieselben führten oft zu bedeutenden Anschwellungen der Gefässscheiden und endlich zu Compression der Gefässe; man konnte alle Uebergänge verfolgen von dieser Verstopfung der perivasculären Räume bis zu haemorrhagischen Zuständen und ausgedehnten Erweichungsheerden des Gehirnes.

Sie werden es vom Standpunkte des Anatomen begreiflich finden, wenn er auf eine Beobachtung, die nach seiner Ueberzeugung klar und unzweideutig ist, Gewicht legt, so lange bis sich zeigt, dass noch andere Verhältnisse hier vorkommen. So lange ich also diesen klaren Befund hatte, war ich der Ueberzeugung, dass dies die einzige Form niederer Organismen wäre, welche bei Diphtherie vorkommt. Ich bezeichnete dieselben als Microsporon diphtheriticum, welches wie das Microsporon septicum, Stäbchen und Micrococcenballen als verschiedene Entwicklungsstadien des gleichen Organismus producirt.

Erst später, nach meiner Uebersiedelung nach Zürich, habe ich anderes Material in die Hand bekommen und wurde mir die Thatsache unwiderstehlich zum Bewusstsein geführt, dass es noch eine andere, ganz differente Form von Diphtherie giebt, welche nur in den gröberen anat. Veränderungen mit jenen ersten Formen der Diphtherie übereinstimmt; es sind das die Fälle von Diphtherie wie sie in Zürich vorkommen und die offenbar ganz identisch sind mit denen, wie sie sich in Basel vorfinden. Sie gehören jener bekannten Form an, in der die sehr rasch sich ausbreitenden Membranbildungen im Pharynx grosse Neigung besitzen, auf die Trachea überzugehen, selbst bevor noch irgend ein erheblicher Zerfall in der Pharynxaffection eingetreten ist. Die Auskleidungen der Trachea, welche

sich hier bilden, müssen als wesentliche Ursache des Todes betrachtet werden, welcher gewöhnlich durch Erstickung herbeigeführt wird. Nur während der Ausbreitung des Localprozesses sehen wir fieberhafte Zustände auftreten, welche dann längere Zeit wieder schwinden könnten, bis schwerere Allgemeinerscheinungen ähnlich den septischen auftreten, welche dem Leben ein Ende machen können, auch ohne Trachealstenose unter den Erscheinungen des Collapses. Der constant dabei vorhandene Eiweissgehalt des Harnes hat an Nephritis denken lassen, doch fehlen tiefere Veränderungen der Nieren und das Krankheitsbild ist nicht dasjenige der Uraemie. Diese Formen, die ich in Prag selten Gelegenheit hatte zu sehen, welche dagegen in der Schweiz und Deutschland vorherrschen dürften, bilden hier also ein ganz einheitliches Bild und es war vom höchsten Interesse, die Organismen genau zu untersuchen, welche sich dabei vorfanden. Der Befund ist ein total abweichender von dem zuerst beschriebenen. Es handelt sich in diesem Falle, wie ich gleich, um das Ding mit einem bestimmten Namen zu bezeichnen, hervorheben will, ausschliesslich um bacilläre Bildungen, während man die früher beschriebenen Formen, zu denen auch meine Prager gehören, als microsporine Diphtheritis bezeichnen kann. Es kann kein Zweifel bestehen, dass hier zwei ganz verschiedene Formen vorliegen.[1]

Ich habe zwei Präparate von demselben Fall aufgestellt, bei schwächerer und mittlerer Vergrösserung. Es handelt sich um zusammenhängende Schichten von Microorganismen, die in der schwachen Vergrösserung eine sehr dunkel gefärbte Linie an der Oberfläche der Exsudatschicht bilden, die nur an Stellen, an denen weiter unten Drüsen liegen, sich in die Drüsenmündungen hinein fortsetzt. Dann kommen Faserstoffschichten, die nur noch in den oberflächlichen Lagen, wie Sie bei stärkeren Vergrösserung sehen werden, schwächer gefärbte Gruppen von solchen Bacillen besitzen. Dann kommen reine Faserstoffschichten, die von mehr oder minder reichlichen Eiterzellen durchsetzt sind. Bei stärkster Vergrösserung (Zeiss $\frac{1}{18}$ Oc. IV) zeigt sich, dass das alles äusserst kurze und schmale Stäbchen sind, die regellos in der oberflächlichen Schicht wie

[1] Nach mündlichen Mittheilungen zweier Collegen aus Russland scheint diese Form in den grossen Epidemieen daselbst die vorherrschende zu sein. Diese Verschiedenheit des Verbreitungsbezirkes ist ein Grund mehr für meine Annahme der gänzlichen Verschiedenheit beider Prozesse.

eingebettet in einer Gallerte zu liegen scheinen. Lenkt man die Aufmerksamkeit hauptsächlich auf solche Stellen, an denen noch fast keine faserstoffige Exsudation vorhanden ist, so kann man sich überzeugen, dass die einzelnen Gruppen von Stäbchen immer in Zellen liegen. Allmählich nehmen diese Stäbchen innerhalb der Zellen zu, sie bilden dichtgedrängte Haufen, die miteinander verschmelzen und schliesslich eine zusammenhängende Membran von Schizophyten an der Oberfläche der Faserstoffschicht bilden.

Was die Form dieser Stäbchen betrifft, so sind sie gleichmässig lang, äusserst schmal und erreichen im Ganzen kaum die Grösse der Tuberkelbacillen. Eine ziemliche Anzahl derselben ist sporentragend und zwar befinden sich stets zwei endständige Sporen in je einem Stäbchen. Bei dem Eintrocknen diphtherischer Membranen, wenn dasselbe allmählich, in gewöhnlicher Temperatur über Schwefelsäure geschieht, vermehren sich die Sporen sehr bedeutend und trifft man dann selten ein Stäbchen, welches keine solchen enthält, viele enthalten bis 4 Sporen.

Welche weiteren anatomischen Veränderungen finden sich nun bei dieser bacillären Diphtheritis im Organismus? Die Beantwortung dieser Frage liefert das zweite Moment von anatomischer Seite zum Beweise des vorher ausgesprochenen Satzes, dass es sich um zwei differente Prozesse handelt. Die gröberen anatomischen Veränderungen in allen unseren Züricher Fällen waren minimal und keineswegs jenem Bilde entsprechend, welches sich bei schweren Infectionskrankheiten herausstellt. In der bei weitem grössten Anzahl der Fälle ist nur eine ganz leichte Milzschwellung vorhanden, in manchen Fällen kann es überhaupt zweifelhaft sein, ob eine Vergrösserung der Milz vorhanden ist. Es fehlen ferner vollständig in allen frischen Fällen jene Veränderungen der Nieren, welche ja sonst bei Infectionskrankheiten regelmässig vorkommen und die, von Virchow mit dem Namen der parenchymatösen Degeneration bezeichnet, schon macroscopisch einen höchst auffälligen Befund darstellen. Es fehlen ebenso durchweg die entsprechenden Veränderungen der Leber und des Herzens, und die Kinder gehen entweder durch Verbreitung des Prozesses auf die Bronchien oder durch entzündliche Veränderungen in den Lungen zu Grunde.

(Nachtrag.) Die Lungenaffection tritt in ihren ersten Anfängen in der Regel in der Form kleinerer, runder bronchopneumonischer

Heerde auf, welche gruppenweise an solchen Stellen zusammenliegen, an denen die zuführenden Bronchien Fibringerinnsel enthalten. Weiterhin entwickeln sich aus diesen Heerden lobuläre Hepatisationen, deren scharfbegrenzte eckige Felder eine dunkelbraunrothe Farbe haben, derb sind und ziemlich glatt auf der Schnittfläche erscheinen. In späteren Stadien können grössere Parthieen eines Lappens in der entsprechenden Weise hepatisirt sein und zwar namentlich in den unteren und hinteren Abschnitten.

Die microscopische Untersuchung dieser pneumonischen Affection zeigt eine ausgebreitete und namentlich um grössere Gefässe sehr mächtige interstitielle Zellablagerung: Interstitielle diphth. Pneumonie.

Der Verlauf dieser Affection scheint ein ziemlich chronischer zu sein, indem man sie in derselben unveränderten Art auch noch in solchen Fällen vorfindet, in denen der eigentliche diphtherische Prozess auf den Schleimhäuten längst geschwunden ist.

Parallel dieser Veränderung gehen die interstitiellen Prozesse in anderen Organen, wie sie namentlich in neuerer Zeit eingehend von Leyden geschildert sind, so die diphth. Myocarditis und Neuritis, welche ich ebenfalls für Spätaffectionen, resp. Nachkrankheiten ansehen möchte.

Eine eingehendere Schilderung verdient dann noch die Affection der Nieren bei der bacillären Diphtheritis. Wie schon bemerkt, erscheinen dieselben in Fällen, welche frühzeitig, nach wenigen Tagen zum Tode führen, nicht wesentlich verändert: die Rinde etwas fleckig geröthet, die Glomeruli in der Regel blass, auch in solchen Fällen, in denen schon reichliche Eiweissmengen im Harn ausgeschieden werden.

Es lag in solchen Fällen, namentlich für mich, der ich die scarlatinöse Glomerulonephritis kennen gelehrt habe, nahe, in den Glomerulis diejenigen Veränderungen aufzusuchen, welche als Grundlage dieser Albuminurie betrachtet werden könnten; allein in keinem einzigen solchen Falle ist es mir gelungen, die Ueberzeugung zu gewinnen, dass diese Organe irgend nennenswerthe Veränderungen darbieten: weder die Veränderungen an dem Kapselepithel, welche Langhans in seinen Fällen nachweisen konnte, noch die Wucherungen der Kerne in der Gefässwandung, welche Friedländer neuerdings bei der Scharlachnephritis beobachtet hat, noch auch die

Wucherungen der Bindegewebszellen innerhalb der Glomeruli, welche ich selbst beschrieben habe und die sehr wohl mit dem Befunde von Friedländer sich vereinigen lassen.

In der That fehlt auch bei der Albuminurie der Diphtheritiker gänzlich jene Eigenthümlichkeit, welche die Scharlachnephritis von Anfang an characterisirt: die Verminderung oder Sistirung der Harnabsonderung. Im Gegentheil wird der Harn in mindestens normaler Menge abgesondert; er erscheint hellgelb, leicht getrübt und reagirt stark sauer. Die Trübungen werden theils durch Harnblasenepithelien, theils durch solche der Nierencanälchen gebildet, welche z. Th. breiten hyalinen Cylindern aufgelagert sind. Die Substanz der letzteren Art von Zellen erscheint grob körnig, verfettet; die Kerne wohl erhalten, gross, bläschenförmig, nicht vermehrt. Vergeblich habe ich in diesen Elementen nach stäbchenförmigen Organismen gesucht, welche etwa denjenigen der diphtherischen Membranen entsprechen könnten. Dagegen enthält die Flüssigkeit zahllose Mengen lebhaft beweglicher Körperchen, welche an die von Hüter und Tommasi im Blute gesehenen Monaden erinnern. Ich vermuthe, dass es sich um Bildungen handelt, welche nicht in die Entwicklungsreihe der eig. diphtheritischen Organismen gehören, sondern nur dann auftreten, wenn Zersetzungsvorgänge in den diphtherischen Membranen oder in dem Gewebe unter denselben stattfinden, die diphtheritische Necrose. Was diese letztere betrifft, so kann ich nicht zugeben, dass dieselbe eine constante und wesentliche Eigenthümlichkeit des diphtheritischen Prozesses ist, am wenigsten aber, dass die diphtheritische Membran aus der Gewebsnecrose hervorgehe. Vielmehr erscheint mir der Zusammenhang zwischen der oberflächlichen Entwicklung der Pilzvegetation und der Necrose durch Störungen der Circulation in den Schleimhautgefässen vermittelt, welche wohl als eine directe Wirkung der Organismen aufgefasst werden müssen. Die oberflächlichen Schleimhautgefässe sind schon in den jüngsten Stadien enorm dilatirt und mit rothen Blutkörperchen vollgepfropft. So lange Circulation in denselben besteht, tritt eine gerinnungsfähige und unter dem Einflusse von Blutferment (weisse Blutzellen) gerinnende Flüssigkeit aus denselben aus, welche die Fibrinniederschläge liefert; später, wenn Stillstand des Blutes, globäre Stase in diesen Gefässen eintritt, kann die Lebensfähigkeit des Gewebes so tief geschädigt werden,

dass ein Absterben desselben eintritt und dasselbe septischen Organismen einen geeigneten Entwicklungsboden darbietet. Gewöhnlich, vielleicht immer, tritt zugleich faulige Zersetzung innerhalb der Faserstofflagen auf. Ich halte desshalb diese necrotisirenden Formen der Diphtheritis, welche Oertel als besondere septische Form beschreibt, für eine Complication des diphtheritischen Prozesses, die in der Regel erst dann eintritt, wenn der letztere abgelaufen oder zum Stillstande gekommen ist.

Die diphtheritische Gefässlähmung scheint mir nun aber nicht nur in dem localen Schleimhautprozesse eine wichtige Rolle zu spielen, sondern ich glaube dieselbe auch für die Folgezustände entzündlicher Natur, welche in inneren Organen auftreten, in Anspruch nehmen zu können. Gerade bei der Nierenaffection tritt dies deutlich hervor, indem hier Albuminurie und Cylinderbildung evident zu einer Zeit eintritt, in der noch keine Spur interstitieller Wucherung vorhanden ist. Indem diese letztere erst später auftritt und zwar gewöhnlich um grössere Gefässstämme und um die Glomeruli herum, kann dieselbe nur, wie in der Lunge, als eine Folge der Gefässläsion betrachtet werden, und wahrscheinlich ist das gleiche der Fall bei der interstitiellen Myocarditis und Neuritis.

Demnach gestaltet sich der Befund bei der bacillären Diphtheritis, soweit die bis dahin vorliegenden Untersuchungen ihn aufgeklärt haben, folgendermaassen:

1. Fibrinöse Exsudation auf den Tonsillen, welche eine grosse Neigung besitzt, auf die Trachea fortzuschreiten.
2. Constante Anwesenheit einer bestimmten Form von Bacillen auf der erkrankten Schleimhaut, welche zuerst in den Epithelzellen sich entwickeln, die fibrinöse Exsudation hervorrufen als Folgezustand einer eigenthümlichen Gefässläsion.
3. Hohes Fieber, welches die Ausbreitung der Schleimhautaffection begleitet, späterhin nachlässt und nur bei septischer Complication sich erneuert.
4. Interstitielle entzündliche Prozesse in inneren Organen als Folgezustände, wie es scheint weniger durch Verbreitung der Organismen im Körper bedingt, als durch ausgebreitete Stö-

rungen im Gefässsysteme (interstitielle Pneumonie, Myocarditis, Neuritis, Albuminurie und interstitielle Nephritis).

5. Faulige Zersetzung der Membranen leitet die septische Complication ein, welche den Tod herbeiführen kann, nachdem der eigentliche diphtheritische Prozess abgelaufen.

Das ist der constante Befund, der sich bei allen diesen Fällen bacillärer Diphtheritis vorfindet. Denjenigen Prozess, den wir früher als Croup bezeichnet hatten, also jene faserstoffige Exsudation, welche, so lange der Prozess nur in vereinzelten Fällen vorkam, mit einer gewissen Wahrscheinlichkeit atmosphärischen Einflüssen zugeschrieben werden konnte, dürfen wir jetzt, wo wir sehen, wie dieser Prozess vollständig endemisch geworden ist, und wir vielfach constatiren können, dass er durch directe Uebertragung fortschreitet, natürlich nicht mehr als eine einfache Entzündung, sondern müssen ihn gleichfalls, wie der anatomische Befund lehrt, als eine Infectionskrankheit betrachten. Wie sehr dieser Prozess von dem früher beschriebenen, der microsporinen Diphtheritis verschieden ist, brauche ich nicht nochmals hervorzuheben, nur einen Punkt möchte ich zur Anregung bringen, damit er von klinischer Seite erörtert werde, das ist die Frage der Lähmungen, die ja in letzter Zeit noch die verschiedenartigste Beurtheilung erfahren hat. Wollen wir derselben näher treten, so wird es nothwendig sein, auch die verschiedenen Prozesse, welche wir unter dem Bilde der Diphtherie subsummirt haben, von einander zu trennen und uns die Frage vorzulegen, ob ein Lähmungszustand in dem einen Falle vorkommt, in dem anderen nicht, oder ob er wenigstens im einen Falle häufiger vorkommt, im anderen spärlicher? Soweit Jemand, der diese Sache nur aus den Mittheilungen von Collegen kennt, urtheilen kann, scheint es mir allerdings, als ob die zuletzt geschilderte Form der Diphtherie viel seltener Lähmungen zu Stande bringt, als die microsporine Form. Es wird vielleicht zweckmässig sein, wenn diese Frage erst auf Grund einer genaueren Statistik zur endgültigen Entscheidung gebracht würde, welche auf der Annahme der differenten Formen der Diphtherie basirt.

Um nicht zu weitläufig zu werden, möchte ich nur noch eine Frage hervorheben, nämlich die eigenthümliche und noch nicht genügend bekannte Thatsache, dass die bacilläre Form der Diphtherie

allerdings noch weitere interne Prozesse nach sich ziehen kann, die wahrscheinlich zusammenhängen mit jenen, von dem verehrten Collegen Gerhardt nur kurz angedeuteten Formen chronischer oder spät eintretender Störungen. Er hat in der That mit vollem Recht an das traurige Ende von Heine, meines früheren Prager Collegen, erinnert. Solche Fälle sind durchaus noch nicht aufgeklärt, in denen nach diphtherischen Prozessen oft längere Zeit nachher, nach Wochen, in denen scheinbar ein volles Wohlbefinden vorhanden ist, wieder schwere Erscheinungen seitens des Darmes auftreten. So ist Heine zu Grunde gegangen, nachdem er mehrere Wochen nach Ueberstehen einer Diphtherie Prag verlassen hatte. Angeblich scheint nur im Darme eine Veränderung gefunden zu sein, welche die behandelnden Aerzte geneigt waren, als eine typhöse zu bezeichnen.

In allen Fällen in Zürich, und das wird Herr College Ziegler wohl auch für seine Beobachtungen in Zürich bestätigen können, kaum mit einer einzigen Ausnahme, zeigten sich merkbare Schwellungen der Peyer'schen Plaques, die meistens das Aussehen haben, welches wir bei Typhus in dem ersten Stadium wahrnehmen können, nämlich eine reticulirte Schwellung. Ich will gleich bemerken, dass es nicht gelungen ist, in diesen geschwellten und stark gerötheten Plaques Organismen nachzuweisen; vielleicht ist es nur ein Fehler der Methode, allein schon die gröbere anatomische Untersuchung lehrt, dass, wenn eine solche Verbreitung stattfindet, sie auf dem Wege des Darmes stattfinden muss. Schon lange war meine Aufmerksamkeit auf Befunde gerichtet, welche hierüber Aufschluss geben könnten; ich erwartete immer, bei Sectionen einmal einen handgreiflicheren Befund zu erhalten, doch ist mir dieses erst in der letzten Zeit gelungen in einem Falle, der mich sehr überraschte, im Magen. Es fanden sich bei demselben ganz brillante diphtherische Auflagerungen und zwar um die Cardia herum. Entsprechend den radiären Faltungen der Schleimhaut fanden sich daselbst länglich ovale Flächen, die ein dunkelrothes Aussehen hatten und den Eindruck machten, als wenn es sich um Geschwüre handelte. Die microscopische Untersuchung zeigte indess, dass dünne Faserstoffschichten an der vom Epithel entblössten Oberfläche der Schleimhaut vorlagen, und in der obersten Schicht dieser Membranen fanden sich dieselben stäbchenförmigen Organismen, wie wir sie auf den Membranen des Pharynx und der Trachea kennen gelernt haben. Ein

Präparat, welches ich aufgestellt, wird Ihnen die vollständige Uebereinstimmung des Befundes darthun. Nach diesem Befunde unterliegt es wohl kaum einem Zweifel, dass es sich um ein Fortschreiten des Processes auf den Magen handelt. Es wächst hiermit die Wahrscheinlichkeit, dass auch die Affection der Peyer'schen Plaques von einer localen Wirkung des Virus abhängt.

Ich habe nun noch eine Frage kurz zu erörtern, nämlich die, ob sämmtliche Fälle von Faserstoff-Exsudationen an der Oberfläche durch diese beiden Formen, welche ich ihres Verlaufes wegen als Diphtherie bezeichnen möchte, repräsentirt werden. Es ist mir ausserordentlich interressant gewesen, dass College Gerhardt einen Fall von Hühnerdiphtherie mitgetheilt hat, der zu Erkrankung zahlreicher Menschen Veranlassung gegeben hat. Allein, ob diese Deutung, welche, wenn ich nicht irre, schon von Bretonneau aufgestellt wurde, richtig, dass die Hühnerdiphtherie identisch mit der Menschendiphtherie ist, möchte ich bezweifeln. Ich habe Gelegenheit gehabt, eine solche Epidemie, allerdings nur in wenigen Exemplaren, zu untersuchen, welche wahrscheinlich ebenfalls durch italienische Hühner eingeschleppt wurde. Die erkrankten Thiere hatten gewaltig dicke, scheinbar fibrinöse Platten auf der Zunge und dem harten Gaumen. In diesem Falle, von dem ich natürlich nicht weiss, ob er identisch ist mit Allem, was man als Hühnerdiphtherie bezeichnet, handelte es sich aber um Prozesse, die in ihrer anatomischen Zusammensetzung gänzlich abweichend waren von jeder menschlichen Diphtherie. Es handelte sich nicht um faserstoffige Exsudationen, wie man allerdings annehmen konnte, indem mächtige Plaques an der Oberfläche liegen, die sich auch mit einiger Mühe abziehen lassen. Aber die Zusammensetzung ist eine andere. Es sind Epithelmassen, zwischen denen rundliche Lücken vorhanden sind, die mit Rundzellen gefüllt sind, also Formen, wie wir sie bei Rinderpest, Variola u. s. w. haben, und neben diesen cellulären Veränderungen findet man riesenförmige Bacillen vor, welche die Grösse der Stäbchen bei der bacillären Diphtherie um das 20—30fache übertreffen. Ich konnte diese interessante Affection leider nur an Spirituspräparaten untersuchen und daher nicht weiter experimentell verfolgen.

Wirklich fibrinöse Exsudationen kommen noch in vielen anderen Fällen vor, in denen parasitäre Pflanzen auf Schleimhäuten vegetiren. Erst in neuester Zeit habe ich solche Fälle gesehen, in denen bei

der Bildung jener bekannten Typhusgeschwüre auf den Aryknorpeln, bei denen es sich, wie Eppinger zeigte, um Ansiedelungen der Typhusbacillen handelt, die in die Knorpel eindringen, an der Oberfläche der Defecte sich Faserstoffschichten vorfanden. Allein wir sehen, dass in diesem Falle der Prozess auf der Schleimhaut nicht continuirlich fortschreitet, und man wird daher gut thun, den Namen der Diphtherie auf diejenigen Fälle faserstoffiger Exsudation zu beschränken, in denen der Process primär auf den Schleimhäuten entsteht, sich auf denselben ausbreitet und die bekannten weiteren Störungen anatomischer und functioneller Art hervorruft. Vernachlässigung der klinischen Erfahrungen würde auch in diesem Falle nur zu leicht neue Verwirrung herbeiführen.

Endlich kommen faserstoffige Exsudationen, die schon mehr den Character der gewöhnlichen Diphtherie an sich tragen, in grosser Ausdehnung vor bei einfachen catarrhalischen Prozessen. Man kann vom anatomischen Standpunkte aus diese catarrhalische Diphtherie vollständig anerkennen, jedoch würde dieser Form ein wesentlicher Theil der klinischen Erscheinungen abgehen, welche wir mit dem Begriff der Diphtherie verbinden. Ueberdies habe ich nicht oft Gelegenheit gehabt, diese Fälle[an] der Leiche zu sehen, weil die Kinder nicht oft zu Grunde gehen; aber schon in Prag habe ich ausgestossene diphtherische Membranen gesehen, welche keine Spur von Stäbchen enthielten und in denen ausschliesslich Micrococcen vorhanden waren, denen entsprechend, die ich als Monaden bezeichnet habe.

Es sind dann von mehreren meiner Schüler einzelne Fälle beschrieben, in denen ähnliche Bildungen an Leichen genauer untersucht werden konnten, indessen alle diese Prozesse entbehren des hohen Grades der Gefährlichkeit, wie die eigentlichen diphtheritischen Prozesse, und will ich daher an diesem Orte nicht weiter darauf eingehen.

Ich bin überzeugt und glaube in der That durch eine ziemlich lange Erfahrung zu dieser Annahme berechtigt zu sein, dass wir nur dann eine echte Diphtherie annehmen dürfen, wenn stäbchenförmige Bildungen in den Membranen vorkommen. In diesem Falle sind zwei Möglichkeiten vorhanden: bei der microsporinen Form werden wir neben ziemlich langen, nicht sporenhaltigen Stäbchen gelbliche Micrococcenballen vorfinden; in diesen Fällen droht eine

rasch sich entwickelnde Allgemein-Infection. In der bacillären Form dagegen, welche durch ihr intensives Fortschreiten auf der Schleimhaut zunächst Gefahr bringt, werden wir eine grosse Menge kleiner Stäbchen antreffen, welche 2 bis 4 Sporen enthalten. Dieselben sind mit etwas Methylenblau so leicht färbbar, dass man sich in der kürzesten Zeit von ihrer An- oder Abwesenheit überzeugen kann, sodass ich also hoffen darf, dass, wenn diese microscopischen Untersuchungen in den einzelnen Fällen am Krankenbette gemacht werden, wir nicht allein zu einer sichereren Prognose kommen, sondern auch zu einer genaueren Statistik, so dass man endlich die zwei Arten der Diphtherie sicher unterscheiden wird. Dann werden sich allmählich die Unklarheiten aufhellen, die noch vorhanden sind, und die sich namentlich auf die therapeutischen Fragen beziehen.

Eine kurze Bemerkung verdient noch die Scharlachdiphtherie. Herr Gerhardt hat seine Ansicht in ausführlicher Weise ausgesprochen; ich besitze verhältnissmässig wenig Erfahrungen darüber. Ich habe die meisten derartigen Fälle gesehen bei meinem ersten Berner Aufenthalte von 1866—1873, wo die diphtheritischen Formen zahlreich neben Scarlatina vorkamen, in denselben Häusern, in denselben Familien, entweder blos Diphtherie oder blos Scharlach, oder beides zusammen. Nun muss ich auch hier übereinstimmend mit Gerhardt erklären, dass ich diese Erkrankung gleichfalls für eine specielle Form halte. Auch ich kann bei diesem Prozesse die zum Theil schon von Brétonneau gemachte Erfahrung bestätigen, dass die Exsudation nicht leicht auf die Trachea übergreift, dagegen vorzugsweise Anschwellung der submaxillaren Drüsen herbeiführt, und zwar nicht jene leichten Formen, wie sie auch bei bacillärer Diphtheritis vorkommen, sondern grössere tumorenartige Bildungen, die sich durch eine grosse Hartnäckigkeit auszeichnen und nicht selten durchweg necrotisch werden, indem sie gänzlich von Micrococcenmassen durchsetzt sind.

Auf weitere Einzelheiten kann ich leider nicht eingehen, da ich Ihre Zeit schon lange genug in Anspruch genommen habe; etwaige Ergänzungen meines Referates kann ich vielleicht im Laufe der Discussion nachtragen.

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